



Essays in Applied Microeconomics

Øystein Hernæs

Thesis submitted for assessment with a view to obtaining the degree
of Doctor of Economics of the European University Institute

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European University Institute
Department of Economics

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Abstract

The thesis uses empirical tools to investigate causal effects in labor economics and politics.

The first chapter analyzes the effects of commercial television in Norway. Matching data on cable television networks with individual-level administrative register data, we find that the expansion of commercial television reduced ability test scores as well as high school graduation rates. We find stronger effects on sons of low-income parents and particularly large effect for children in elementary school. Aggregate data show a substantial drop in time spent reading by young people in the same period, suggesting that television watching may have crowded out more cognitively stimulating activities such as reading. This chapter is joint work with Simen Markussen and Knut Røed.

The second chapter investigates how sickness absence behavior in Norwegian municipalities was affected by the terrorist attack in Norway on July 22, 2011. Using register data covering the complete Norwegian population, I find that sickness absence rates declined substantially in municipalities affected more intensely by the attack. In municipalities from which a resident was killed in the attack, sickness absence rates declined by 4 % compared to municipalities without victims. The effect is precisely estimated, stable across several challenging specifications, and persists for as long as there is available data. The effect for people in their 20's is more than twice that for the population at large -- for this group, local exposure to the attack decreased absence rates by around 10 %.

The third chapter exploits a natural experiment in the Norwegian political system. I find that obtaining the right to vote at a lower age is associated with substantially higher turnout among first-time voters, and that this is driven by parental influence. Counter to conventional wisdom about the habitual nature of voting, this difference in political participation does not persist for subsequent elections.

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Chapter I

Television, Cognitive Ability, and Early Career Outcomes¹

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Abstract

We exploit heterogeneity in the growth of commercial television across Norwegian municipalities to estimate the effect of television coverage on standardized ability (intelligence) test scores and early career outcomes of young men. Matching a dataset containing the universe of Norwegian cable television networks up to 2004 with individual-level administrative register data, we find that the expansion of commercial television reduced ability test scores as well as high school graduation rates, and increased the probability of receiving disability benefits. Our results indicate that one year of living in a municipality with full coverage of cable television during childhood and adolescence lowers ability test scores of young men by 0.75 % of a standard deviation, corresponding to around 0.1 IQ points. Although television has negative effects on cognitive ability at all ages, the effect is larger for children in elementary school. Aggregate data show that this period coincided with a substantial drop in time spent reading by young people, suggesting that television watching may have crowded out more cognitively stimulating activities such as reading.

Keywords: Human capital, Media
JEL Classification: J13, J24, L82

¹ This paper is part of the project “Social Insurance and Labor Market Inclusion in Norway”, funded by the Norwegian Research Council (grant #202513). Administrative registers made available by Statistics Norway have been essential. Data on ability scores have been obtained by consent from the Norwegian Armed Forces, who are not responsible for any of the findings and conclusions reported in the paper.

1 Introduction

Since television began to be introduced to a large audience around the mid-20th century, its effects have been debated. The concern has largely been that television encourages a particularly passive form of engagement, and thus may be damaging to intellectual development. This view has been argued by social commentators (e.g. Postman (1985)), but has also received support in professional circles: American pediatricians have concluded that television affects children's cognitive abilities negatively (American Academy of Pediatrics (2001)). The complete opposite view has also found supporters: Johnson (2006) argues that popular culture, including television, has become more complex and intellectually demanding over time and that this gives beneficial cognitive payoffs.

Cognitive skills are essential factors underlying individual (Griliches and Mason, 1972; Cunha and Heckman, 2007) and aggregate economic outcomes (Hanushek and Woessmann, 2008). Since its introduction, television has spread all over the world, hence its effects are also likely to be ubiquitous. If television viewing is indeed harmful to the development of cognitive skills, that is critical knowledge for policy makers and families alike. In this paper, we examine how a geographically staggered expansion of access to commercial cable television in Norway affected children's cognitive skill developments and early career outcomes. We provide empirical support for the view that commercial cable television indeed has negative effects on cognitive skills – as measured by intelligence test scores at time of military service enrolment – and that it also reduces high-school completion rates. In addition, we find indications that a large supply of commercial television during childhood raises the probability of becoming a disability benefit claimant later on. To the best of our knowledge, our paper is the first to analyze the effects of television on later long term educational and labor market outcomes

When television was deregulated in Norway in the beginning of the 1980's, it became legal to forward television signals broadcast by satellites in local cable networks, and a large roll-out of cable networks was initiated. We argue that the growth in cable networks was a supply-led development driven by geographical factors and settlement patterns. To account for unobservable factors possibly related to both television adoption and outcomes, we include municipality fixed effects and county-year fixed effects and time-varying socio-economic variables at the municipality level in our regressions. Further, due to the richness of our data, we are able to control for relevant parental characteristics in order to improve precision, account for possible selection, and specify treatment interactions.

We match a dataset containing the universe of Norwegian cable television networks at the municipality level up to 2004 with comprehensive individual-level administrative register data. Our outcomes of interest are standardized ability (intelligence) test scores and early educational and labor market outcomes of the male Norwegian population.

Our results indicate that one year of living in a municipality with full coverage of cable television during childhood and adolescence lowers ability test scores of young men by 0.75 % standard deviations, corresponding to 0.11 IQ points. It reduces the high-school completion rate by a little less than half a percentage point. We hypothesize that these effects may be driven by consumption of television crowding out more cognitively stimulating activities such as reading, in which there was a substantial, aggregate drop in the same period.

2 Literature

The pediatrics literature has documented a negative association between television viewing and a wide range of health outcomes (Strasburger et al., 2010). Jolin and Weller (2011) lament the lack of longitudinal research designs, but conclude that current research suggests that television viewing has negative effects. A review of short-term experimental studies reports mixed results of television content, though more fundamentally raises the concern about the external validity of such studies (Thakkar et al., 2006).

The last decade has seen a rapidly growing literature in economics on the effects of media. Television in particular has been found to impact outcomes as diverse as voter turnout in the US (Gentzkow, 2006), party choice in the US and Russia (DellaVigna and Kaplan, 2007; Enikolopov et al., 2011; Martin and Yurukoglu, 2014), social capital in Indonesia (Olken, 2009), fertility, women's status and school enrollment in India (Jensen and Oster, 2009), and divorce and fertility in Brazil (Chong and Ferrara, 2009; La Ferrara et al., 2012).

The study most closely related to ours is Gentzkow and Shapiro (2008), who analyze the effect of the introduction of television in the US in the 1940's and 50's on standardized test scores. They find that contrary to many worries, television exposure during pre-school age not only did not lower test scores, but in fact raised reading and general knowledge scores for socially disadvantaged groups. On the basis of this effect heterogeneity, they conclude that "the cognitive effects of television exposure depend critically on the educational value of the alternative activities that it crowds out (p. 282)." This makes it pertinent to reexamine the issue, since most societies have undergone large changes in their educational and home environments since the early post-war era and one may suspect that TV is crowding out more

valuable activities today. We also complement their study in that we are able to analyze effects at different ages and on longer-term outcomes.

Earlier research has identified a decline in average test scores during the 1990's (Sundet et al., 2004). As shown in Figure 1, our own data confirm this development, and also indicate that the negative trend has continued after the turn of the century. In light of the evidence suggesting negative effects of television, one explanation may be the growth in commercial cable television.

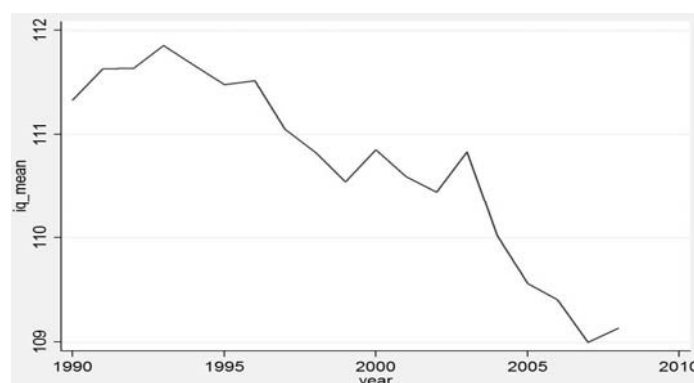


Figure 1. IQ of Norwegian conscripts 1990-2008. The test was normed on the test cohort of 1954, which provides the reference point (i.e. 1954 mean = 100).

3 Data and institutional setting

3.1 Cable television in Norway

Television was introduced in Norway in the 1960's. Until 1981, the state-controlled Norwegian Broadcasting Corporation held a legal monopoly on broadcasting in the country, and for most Norwegians, only a single TV-channel was available. In December 1981, the newly elected government announced that 30 other agents would obtain broadcasting licenses the following year, thereby breaking the public monopoly. It then became legal to forward television signals broadcast by satellites in local cable networks. All such local cable networks had to register with the Post- and Telecommunications Authority. The legalization in 1981 initiated a large-scale roll-out of local cable networks. Because of economies of scale in laying the necessary cables, the roll-out took place primarily in densely populated areas (Norwegian Ministry of Culture, 1995). Mandatory registration continued until 2004, at which point 40 % of the population was covered. The aggregate evolution of the number of households covered is shown in Figure 2.

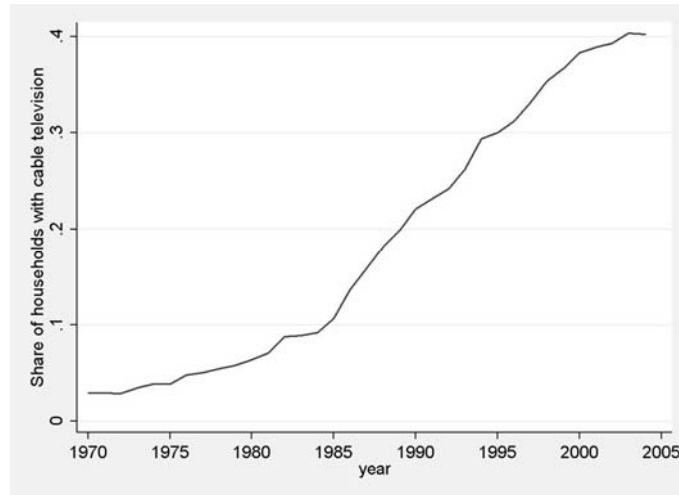


Figure 2. Share of households with cable television 1970-2005

The television data consist of the universe of Norwegian local cable networks up to 2004. They contain more than 11 000 unique networks, each with the number of households covered, first date of operation, and municipality. We combine this with data on the number of households in a municipality to obtain the yearly coverage rate in each municipality up to 2005. The maps in Figure 5 show six snapshots of coverage across the country between 1980 and 2005. They illustrate the considerable geographical disparities in the roll-out process, with cable networks first established in the Oslo-area, and then expanded to other densely populated areas throughout the country. To arrive at individualized exposed-to-TV-variables, we construct for each person born between 1974 and 1987, the cumulative cable TV coverage over their first 18 years in their municipality of birth. The resultant TV coverage variable thus varies from zero (for individuals who never had access to cable TV before age 18) to 18 (for individuals who had access throughout their childhood and adolescence).

We do not have the same kind of data on coverage by parabolic antennas. However, there are two reasons why this is not a concern: First, parabolic antennas were providing only a small amount of television coverage in this period – when Statistics Norway started their media survey in 1991, only 5 % of households had a parabolic antenna. Second, since parabolic antennas were a substitute for cable, our “control group” actually had more television coverage than we assign them, which would tend to attenuate our estimates.

3.2 Individual level data

The rest of the data come from administrative registers. Our measure of cognitive ability is based on data from the Norwegian Armed Forces. Before military service, all Norwegian men

undergo an assessment of suitability when they are 18 years old. From 1969/70, this assessment has included intelligence testing. All test-takers receive a score which is a composite of three tests, on arithmetic, word similarities and figures. This score is reported in stanine units, a measure that has a standard deviation of 2.²

In addition to ability test scores, we apply outcomes intended to measure early career outcomes. These include a dichotomous variable indicating high-school completion at age 21 (two years after the “normal” completion age). Moreover, we use dichotomous variables indicating education or employment at age 21 and disability benefit reciprocity at age 21, respectively.

We match the various outcomes with data on an individual’s municipality of birth and his parents’ characteristics. The first cohort in our sample is the cohort born in 1974, which is the first year from which we have data on residence at birth. We include all subsequent cohorts until and including the one born in 1987, which turned 18 years old in 2005. The reason why we stop at the 1987 birth cohort is that we do not have the ability to compute total coverage for later cohorts, as the mandatory registration of networks ended in 2004. To sum up, our sample consists of the birth cohorts 1974-1987, with outcomes measured in 1992-2005 (ability test scores) and 1995-2008 (early career outcomes). To allay worries concerning an increasing share of people with immigrant background taking the test, we include only men born in Norway to Norwegian-born parents.

Tables 1 and 8 present a descriptive overview of the data we use in this paper, for the analysis population at large (Table 1), and for selected birth cohorts (Table 8). In total, we have around 311,000 observations. On average, the persons in our dataset had access to cable TV in only 3.45 years during their childhood and youth. But TV exposure increased considerably over time, from less than 2 exposure-years for our first cohorts to more than 5 for our last. The control variables we are going to use in our statistical analyses are defined at the individual or at the municipality-cohort levels, in both cases measured no later than the time of birth. In addition, we will in some cases use municipality characteristics measured in 1980 (pre-legalization) interacted with time-trends or time-indicators. Individual characteristics comprise the educational attainment and earnings levels of both parents, whereas municipality characteristics cover socio-economic factors, such as average education, employment, and

²For expositional purposes, we sometimes refer to the test scores results in terms of IQ scores. When converting the stanine scores, which has a mean of 5 and a standard deviation of 2, to the IQ scale, which has a mean of 100 and standard deviation of 15, we follow the practice of assigning a stanine score of 5 an IQ score of 100, and add/subtract $15/2 = 7.5$ for each 1 stanine point upwards/downwards deviation from 5 (Kristensen and Bjerkedal, 2007; Brinch and Galloway, 2012). The IQ score is thus calculated according to the formula $IQ = 100 + (stanine - 5) * 7.5$.

earnings; see the tables for details. Most of the variables have been relatively stable across cohorts, with the exceptions of parental earnings and women's employment.

Table 1. Descriptive statistics

	mean	sd	min	max
Outcomes				
ability test score at age 18	5.17	(1.74)	1.00	9.00
high school completed at age 21	0.67	(0.47)	0.00	1.00
in work or education at age 21	0.87	(0.33)	0.00	1.00
disability reception at age 21	0.03	(0.18)	0.00	1.00
Explanatory variable				
TV, years of coverage until age 18	3.45	(3.70)	0.00	18.00
Controls				
Individual-level				
education father, four levels	1.99	(0.83)	1.00	4.00
education mother, four levels	1.83	(0.73)	1.00	4.00
income father, 1000 USD	69.11	(30.44)	0.00	259.09
income mother, 1000 USD	20.94	(22.87)	0.00	259.09
Municipality-level				
schoolyears attained	11.44	(0.44)	8.90	12.84
income males, 1000 USD	62.57	(8.17)	31.39	88.98
income females, 1000 USD	25.52	(7.30)	6.58	45.94
employment rate males	0.80	(0.04)	0.42	0.90
employment rate females	0.44	(0.12)	0.08	0.71
schoolyears attained 1980	11.45	(0.41)	10.14	12.63
income males 1980, 1000 USD	64.43	(7.28)	38.75	82.53
income females 1980, 1000 USD	25.88	(5.51)	12.72	37.48
employment rate males 1980	0.81	(0.03)	0.56	0.89
employment rate females 1980	0.45	(0.09)	0.18	0.62
Observations	310587			

Note: Parental education in four levels (less than high school, completed high school, university bachelor, university master); income is yearly earnings measured in 2013 prices, converted to USD with an exchange rate of 1 USD=6.5 NOK and right-censored at 20 times the "basic amount" used by the Norwegian Social Insurance Scheme; municipality level income levels is calculated based on inhabitants aged 19-66; in calculating employment rates, we count as employed individuals with yearly earnings at least 2 times the basic amount, corresponding to around USD 26 000.

4 Empirical strategy

In order to identify the causal impacts of cable network expansion, we use the municipality-by-cohort specific coverage rate described in Section 2.1 as the key explanatory variable for the outcomes described in Section 2.2. The identifying assumption is then that the geographical roll-out of cable TV was as good as randomly assigned with respect to other factors that could have generated local variations in the developments of cognitive ability and other outcomes across cohorts. We argue that this assumption is likely to be satisfied, as there is evidence that the cable network expansion during the 1980's and 1990's was solely supply side driven. Building cable networks required heavy investment in infrastructure, and was only profitable in densely populated areas. Given that there was a large excess demand for cable TV everywhere, the actual expansion pattern was determined by economies of scale and physical/topological constraints. Participants from the supply side of the cable television market in this period have said that it is hard to see any factors other than suppliers' capacity and population density that had an impact on where networks were built, and that after deregulation, suppliers were suddenly allowed to cater to a demand that had been present for a long time.³ Further evidence for the exogeneity of coverage comes from considering the size and distribution of the networks. Many networks in a locality were constructed at the same time as parts of the same development. The average number of new households gaining access conditional on expansion in any given municipality-year, which is our observational unit, is 691. Thus it is clear that access was determined at a quite different level than the household. The fact that cable networks did not operate in almost 40 % of municipalities (163 of 430) also shows that whether to obtain access was clearly not an individual-level decision.

In 1999, a government White paper concluded that “significant development of cable networks beyond today's level will most likely not be profitable (Norwegian Ministry of Culture, 1999, ch. 2.2),” and that “one does not expect significant further development of cable utilities beyond today's coverage of around 38 % (Norwegian Ministry of Culture, 1999, ch. 2.3).” From Figure 2 we see that this assessment proved correct. The report cites topographical and physical barriers as reasons for why full coverage would not be possible.

Although demand did not play a significant role for the roll-out of cable TV in Norway, we cannot a priori rule out that the (typically densely populated) areas selected for early

³ Former head of the union of commercial cable-TV operators in Norway, Knut Børmer; personal communication. Terje Frøsland, of the state owned *Norwegian Telecommunications (Televerket, from 2005 Telenor)*; personal communication.

expansion have been characterized by different developments in ability scores and early adult outcomes than other areas. Hence, as we detail in the next subsection, an important element of our empirical strategy is to examine our findings' robustness with respect to the inclusion of various sets of control variables, also allowing for differential local trends.

To assess more generally the likelihood of spurious trends, we examine in Figure 3 the pre-treatment trends in the cognitive ability test scores. Since all cohorts with available municipality of birth are treated, we cannot construct pre-trends by municipality of birth. However, what we can do for earlier cohorts is to construct pre-trends by municipality of residence in the test year (at age 18). We consider the test years 1974-1985 (equivalently, the birth cohorts of 1956-1967). We divide municipalities into three groups: One group consisting of municipalities that never had any cable television coverage at all, the two others created by grouping together municipalities with increase in cable television coverage between 1980 and 2000 below or above the median increase in this period. Because of a renormalization of the scoring that was undertaken by the test administrators around 1980, we want to take out annual variation, and plot in Figure 3 the predicted residuals from a regression of test scores on a series of test year dummy variables. Though there is a difference in levels, it is reassuring that the three groups seem to follow a very similar development over time.

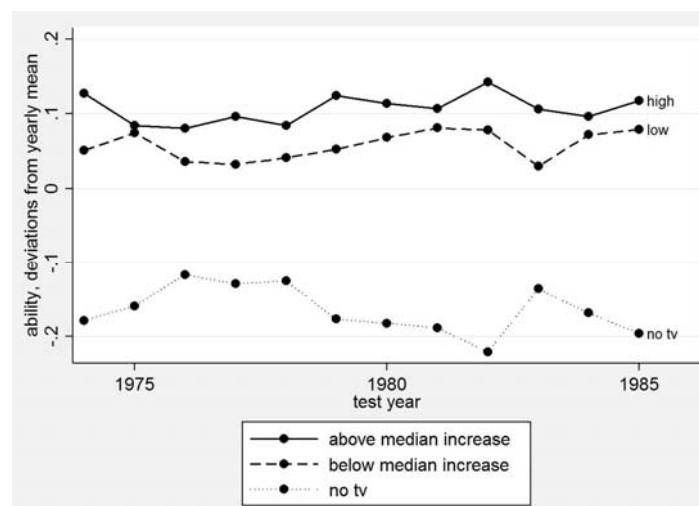


Figure 3 IQ of Norwegian conscripts tested 1974-1985 by increase in television coverage 1980-2000 in municipality of residence, deviations from yearly mean.

To empirically assess the impacts of cable TV coverage, we use linear regression models with the various outcomes measured at age 18 or age 21 as dependent variables and the number of years with cable TV coverage until age 18 as the central explanatory variables. The

latter vary at the municipality-cohort level only. And since residence decisions may respond endogenously to the supply of TV channels, we fix individuals to their municipality of birth and assign them that municipality's subsequent television coverage. This method clearly introduces some measurement error. It implies that we estimate the intention-to-treat effect at the baseline.

In the most restrictive version of the model, we include as control variables municipality fixed effects (δ_m) and county-year fixed effects (Θ_{ct}). The municipality-fixed effects are included to control for any time-invariant factors that vary between municipalities in a way that correlates with cable TV expansion. We can obviously not use municipality-year fixed effects, as that would totally absorb also the effects of interest. However, by including county-year fixed effects we control for time variations that are common within counties. There are 19 counties in Norway, and on average each county covers approximately 23 municipalities. A somewhat unfortunate consequence of using county-year fixed effects is that we effectively throw out observations from Oslo, the capital city, which is by far the largest municipality and forms its own county. However, we exploit these data in a separate analysis, where we use observations from the largest cities/municipalities only (without county-year dummy variables).

To account for socio-economic developments that potentially vary across municipalities, we include a covariate vector m_{mt} consisting of municipality-level average education, male and female income, and male and female employment rate. These are all measured in the year of each cohort's birth; hence they are not absorbed by the municipality-fixed effects. To arrive at what we label our "baseline model", we further add a vector of parental characteristics. Family background is known to be important for young people's outcomes; hence we can use these variables to reduce residual variance. We include a vector of parental characteristics p_{it} consisting of parental income and indicator variables corresponding to four different categories of completed education (less than high school, high school, university bachelor, university master or above). As with the municipality covariates, the parental characteristics are measured at the year of birth of the individual.

This gives the following regression equation:

$$a_{imt} = \beta TV_{mt} + \delta_m + \Theta_{ct} + m_{mt} \zeta + p_{it} \eta + \varepsilon_{imt}, \quad (1)$$

which we estimate with OLS, with standard errors clustered at municipality. a_{imt} denotes an individual's ability score, while TV_{mt} is an individual's cumulative cable television coverage

since the year of birth. Since TV_{mt} is measured in years, the coefficient will be interpretable as effect per year of coverage.

Finally, as an additional check for spurious local trends, we interact the vector of pre-reform (1980) municipal characteristics with either a linear time trend or with year dummy variables.

5 Main results

Our main estimation results are reported in Table 2. The first row shows the results from the most restrictive model, where we only control for municipality and county-year fixed effects. With the cognitive ability score as the outcome, this specification gives an estimated coefficient on our television exposure measure of -0.015. To put this in perspective, we recall that the stanine scale has a standard deviation of 2, thus 0.015 constitutes 0.75 % of a standard deviation. Since the IQ scale has a standard deviation of 15, this estimated effect of one additional year of full television coverage would roughly correspond to a 0.11 points reduction in IQ. By comparison, Brinch and Galloway (2012) estimate the effect of one year of schooling to be around 3.7 IQ points using an education reform in Norway in the 1960's. Taken at face value, our estimate thus indicates that 18 years of full cable TV coverage has a negative impact on cognitive ability comparable to half a year's schooling. Carlsson et al. (forth.) use a quasi-experimental setting in Sweden to exploit variation in test taking date for young Swedish males preparing for military service. Their results imply that one year of schooling raises crystallized (synonyms and technical comprehension) test scores by around 20 % of a standard deviation, corresponding to around 3 IQ points, but has no effect on fluid (spatial and logic) intelligence tests.

Moving on to the other outcomes, our results indicate that one extra year of cable TV coverage reduces the probability of high-school completion by age 21 by around half a percentage point, and raises the probability of claiming a disability benefit by 0.1 percentage point. The effect on disability benefits suggests that how physical health is impacted may play a role. We do not find statistically significant evidence of effects on the overall probability of being in work or in education at age 21, though the point estimate is consistently negative, indicating that the high school dropouts are not finding gainful opportunities elsewhere. To again put the results in perspective, we note that the estimated effect on high-school completion is similar in size to the corresponding "effect" of one additional year of parental education, as reported by Bratberg et al. (2012, Figure 5).

As we move downwards in Table 3, adding more and more control variables, it is reassuring that the estimates remain stable. Including time-varying municipality-level and parental covariates changes the estimated effects very little, and – if anything – raises the estimated effects of TV exposure.

An important assumption so far has been that municipalities belonging to the same county would have followed a common development in time in the absence of cable television expansion. This assumption needs to be challenged. One way to do this would be to allow for separate linear time trends in each municipality. However, since the treatment is an 18 year cumulative of previous coverage in the municipalities, the expansion variables also almost follow linear trend patterns; hence including municipality-specific linear time trends would make identification infeasible. A better alternative is to allow municipalities to follow different time trends based on their pre-television legalization characteristics. We therefore interact the 1980 levels of the municipality covariates – education level, male and female income and unemployment rate – with either linear time trends or year dummies. The results from these two specifications are shown in the fourth and fifth rows of results in table 3. The estimated coefficients are somewhat reduced, but remain of the same magnitude and are still precisely estimated.

Since the expansion of cable TV largely was heavily influenced by population density, one may worry that differential outcome trends in areas with different population density may distort our results. For example, we could imagine that the development of cognitive abilities has been more negative in densely populated areas for reasons that have nothing to do with cable TV expansion. To assess this potential source of bias, we include in the final row of Table 3, linear time trends interacted with indicators of deciles of municipality population and population density measured in 1980. Standard errors increase as expected, but the coefficients hardly change at all.

Our overall judgment is that estimated effects of cable TV expansion are highly robust with respect to model specification and the inclusion/exclusion of various groups of explanatory variables.

Table 2. Main results – Effects of television exposure

	Ability	High School	In work or	Disability
		graduate	education	recipient
Aggregate fixed effects	-0.0151 (0.0045) ***	-0.0044 (0.0011) ***	-0.0003 (0.0008)	0.0010 (0.0004) ***
Aggregate fixed effects + municipality covariates	-0.0161 (0.0045) ***	-0.0054 (0.0011) ***	-0.0010 (0.0008)	0.0014 (0.0004) ***
Aggregate fixed effects + municipality covariates + parental covariates (Baseline)	-0.0182 (0.0041) ***	-0.0062 (0.0011) ***	-0.0012 (0.0008)	0.0015 (0.0004) ***
Baseline + Pre-reform demographics #time trend	-0.0119 (0.0043) ***	-0.0043 (0.0012) ***	-0.0010 (0.0008)	0.0011 (0.0004) **
Baseline + Pre-reform demographics #time FE	-0.0115 (0.0043) ***	-0.0042 (0.0012) ***	-0.0010 (0.0008)	0.0011 (0.0004) **
Baseline + Population and urbanity groups #time trend	-0.0101 (0.0053) *	-0.0040 (0.0013) ***	-0.0016 (0.0009) *	0.0010 (0.0005) *
ymean	5.17	0.67	0.87	0.03
N obs	280096	310584	310587	310587
N municipalities	430	430	430	430

Note: “Aggregate fixed effects” includes municipality fixed effects and county-year fixed effects; “municipality covariates” includes average years of schooling attained, male and female income levels, male and female employment rates; “parental covariates” includes education level fixed effects and mother’s and father’s income. “Baseline” includes all of the preceding. “Pre-reform demographics” includes 1980 levels of the municipality covariates. “Population and urbanity groups” includes fixed effects for the deciles of municipality population and population density measured in 1980. When time trends are specified, only year fixed effects are included in stead of county-year fixed effects. Standard errors clustered on municipality. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6 Alternative models and robustness checks

6.1 Restriction to the largest cities and municipalities

As pointed out in Section 3, the use of county-year fixed effects in our main empirical analysis implies that we have not (so far) exploited data from Oslo, the capital. This is unfortunate, as Oslo has been in the forefront of the cable TV expansion development. As an alternative approach, we now focus exclusively on the most heavily populated areas in Norway; i.e., we construct two samples; one consisting of persons in the five largest cities (Oslo, Bergen,

Trondheim, Kristiansand, Stavanger), and one consisting of the ten most populous municipalities in Norway. Using these samples answers two concerns: One is that there may have been unobserved factors impacting cognitive skills and educational and labor market outcomes of young people in large cities or urban regions in this period, and that this is picked up by our television measure, since the expansion largely took place in populous areas. A separate concern is that the expansion to some extent was demand-driven after all, and that the demand for cable TV could be correlated with the trends in our outcome measures. Though this may be a valid concern for very small municipalities, such a mechanism would be an unlikely factor to drive the results in a comparison between very large municipalities.

The results are shown in Table 3. Apart from dropping the county-year dummy variables, the model specification is the same as for the baseline model in Table 3. Standard errors (still clustered at the municipality level) blow up with the reduced sample size, but the estimated effects remain quite stable. In particular for the ability test score, the coefficients are almost identical as in the baseline specification.

Table 3. Restriction to the largest cities and municipalities

	Ability	High School graduate	In work or education	Disability recipient
5 largest cities	-0.0114 (0.0140)	-0.0004 (0.0034)	-0.0024 (0.0043)	-0.0005 (0.0030)
ymean	5.44	0.70	0.86	0.03
N obs	60780	66711	66713	66713
N municipalities	5	5	5	5
10 largest municipalities (population in 1980>30000)	-0.0176 (0.0101)	-0.0061 (0.0029) *	-0.0023 (0.0022)	0.0006 (0.0012)
ymean	5.41	0.69	0.86	0.03
N obs	79006	87125	87127	87127
N municipalities	10	10	10	10

Note: All models include municipality fixed effects, year fixed effects, municipality covariates (average years of schooling attained, male and female income levels, male and female employment rates) and individual-level covariates (education level fixed effects and mother's and father's income). Standard errors clustered on municipality. Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

6.2 Family fixed effects

A particularly reliable source of identification of cable TV effects would be to compare brothers who have been subjected to different exposure intensities due to differences in age. Identification with family fixed effects will in practice only come from younger brothers

having higher television exposure, thus we include fixed effects for sibling order in addition to family fixed effects to be sure not to pick up birth order effects. When running this demanding specification the point estimates are not statistically significant, ref. Table 4. The point estimates for ability and high school graduation are almost identical to those from the baseline specification, while those for the labor market outcomes differ.

Table 4. Family fixed effects

	Ability	High School graduate	In work or education	Disability recipient
TV	-0.0172 (0.0175)	-0.0051 (0.0043)	0.0029 (0.0041)	-0.0001 (0.0017)
ymean	5.17	0.67	0.87	0.03
N obs	280096	310584	310587	310587
N families with >1 son	98235	109887	109887	109887

Note: All models include municipality fixed effects and county-year fixed effects. Standard errors clustered on municipality. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6.3 Parental selection into treatment

Even though we measure television exposure at the baseline (municipality of birth), we may be concerned that would-be parents have already selected into certain municipalities before having children. If this generated an increasingly negatively selected population in TV-intensive municipalities, this would distort our estimates. To investigate this, we perform placebo regressions using as dependent variables the (pre-determined) parental covariates we use as control in our baseline specification.

The results are shown in Table 5. If anything, the observed relationship is positive, in the sense that future television coverage is somewhat positively correlated with parents' education and income in the offspring's year of birth, though these correlations are not big, and also largely disappear when including the time trends/dummies interactions with the pre-legalization municipality-level characteristics.

Table 5. Placebo regressions – parental characteristics in year of birth and future television exposure

	Father			Mother		
	High School degree	University degree	Income	High School degree	University degree	Income
Aggregate fixed effects	0.001	0.003	0.332	0.002	0.003	0.035
+ municipality covariates	(0.002)	(0.001) **	(0.124) ***	(0.001)	(0.001) **	(0.080)
Aggregate fixed effects	-0.000	0.001	0.091	0.001	0.002	-0.111
+ municipality covariates	(0.002)	(0.001)	(0.105)	(0.001)	(0.001) **	(0.080)
+ Pre-reform demographics						
#time trend						
Aggregate fixed effects	-0.000	-0.002	-0.060	0.001	0.002	-0.118
+ municipality covariates	(0.002)	(0.001)	(0.107)	(0.001)	(0.001) **	(0.079)
+ Pre-reform demographics						
#time FE						
Aggregate fixed effects	-0.000	-0.002	-0.060	-0.002	-0.003	-0.179
+ municipality covariates	(0.002)	(0.001)	(0.107)	(0.002)	(0.001) **	(0.093) *
+ Population and urbanity						
#time trend						
ymean	0.39	0.19	66.05	0.29	0.16	23.82
N obs	194469	194469	194469	194558	194558	194558
N municipalities	430	430	430	430	430	430

Note: “Aggregate fixed effects” includes municipality fixed effects and county-year fixed effects; “municipality covariates” includes average years of schooling attained, male and female income levels, male and female employment rates. “Pre-reform demographics” includes 1980 levels of the municipality covariates. “Population and urbanity groups” includes fixed effects for the deciles of municipality population and population density measured in 1980. Standard errors clustered on municipality. Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

7 Mechanism

Since we do not have geographically disaggregated data on television watching, we can only estimate the intention-to-treat effect of television coverage; however we can shed some light on the likely mechanism by analyzing heterogeneous effects and looking at aggregate data.

7.1 Heterogeneous effects by socioeconomic status

We analyze whether there is heterogeneity in the response based on individuals’ background. We focus on differences in economic resources and socioeconomic status, as measured by parental income. Parental resources may have implications both for the extent to which children’s TV consumption is regulated/monitored, and for the kind of alternative activities it substitutes for.

In table Table 6 we estimate the baseline model by household income quartile, where income quartile is defined at the municipality level. The negative effects are comparable across quartiles, but largest for the lowest-income households, and smallest for the

highest-income households. These results suggest that more resourceful families may be in a better position to regulate their children's consumption of television.

Table 6. Effect of television exposure by income quartiles

	Ability	High School graduate	In work or education	Disability recipient
Q1	-0.0264 (0.0082) ***	-0.0074 (0.0024) ***	-0.0037 (0.0018) **	0.0020 (0.0009) **
ymean	4.90	0.57	0.83	0.04
N obs	68981	77654	77656	77656
Q2	-0.0152 (0.0078) *	-0.0053 (0.0020) ***	-0.0013 (0.0016)	0.0020 (0.0010) *
ymean	4.98	0.64	0.87	0.03
N obs	70204	77635	77636	77636
Q3	-0.0264 (0.0075) ***	-0.0063 (0.0024) **	-0.0007 (0.0015)	0.0015 (0.0009) *
ymean	5.19	0.69	0.88	0.03
N obs	70093	77652	77652	77652
Q4	-0.0071 (0.0072)	-0.0059 (0.0021) ***	-0.0009 (0.0014)	0.0005 (0.0008)
ymean	5.58	0.77	0.90	0.02
N obs	70818	77643	77643	77643

Note: All models include municipality fixed effects county-year fixed effects, municipality covariates (average years of schooling, male and female income levels, male and female employment rates) and individual-level covariates (education level fixed effects and mother's and father's income). Standard errors clustered on municipality. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

7.2 Critical periods

We also split television exposure in three periods of six years each, roughly corresponding to the pre-school, elementary, and middle and high school periods. The effect is negative across ages, and largest in the period between ages 6 and 12, ref. Table 7. This finding corresponds well to the results of van den Berg et al. (2014), who find that the period around age 9 is critical for the development of adult cognitive ability and mental health, and suggests looking at something going on at this age for further evidence of what exactly television viewing crowded out.

Table 8: Effect of television exposure at different ages. All models include municipality fixed effects county-year fixed effects, municipality covariates (average years of schooling, male and female income levels, male and female employment rates) and individual-level covariates (education level fixed effects and income for mother and father). Standard errors clustered on municipality. Standard errors in parentheses.

Table 7. Effect of television exposure at different ages

	Ability	High School graduate	In work or education	Disability recipient
TV 0-5	-0.0133 (0.0069) *	-0.0041 (0.0020) **	-0.0008 (0.0013)	0.0011 (0.0007)
TV 6-12	-0.0248 (0.0088) ***	-0.0098 (0.0026) ***	-0.0022 (0.0019)	0.0023 (0.0009) **
TV 13-18	-0.0143 (0.0119)	-0.0027 (0.0032)	0.0004 (0.0025)	0.0003 (0.0014)
ymean	5.17	0.67	0.87	0.03
N obs	280096	310584	310587	310587
N municipalities	430	430	430	430

Note: All models include municipality fixed effects county-year fixed effects, municipality covariates (average years of schooling, male and female income levels, male and female employment rates) and individual-level covariates (education level fixed effects and mother's and father's income). Standard errors clustered on municipality. Standard errors in parentheses.* p<0.10, ** p<0.05, *** p<0.01.

7.3 Evidence from time use surveys

Here we use data from time use surveys to look at how time spent on selected leisure activities have developed at an aggregate level in the time period that we are analyzing. Figure 4 shows that young men increased their time spent watching television throughout the end of the 20th century. Two activities that saw precipitous declines in the period in which television viewing increased most rapidly, 1990-2000, were reading and sports and outdoor activities. The shifts along the extensive margin were also dramatic. It is likely that the negative effects of television may operate through both less “mental exercise” from reading and a direct health effect, although from these aggregate figures, the decline in reading appears more important.

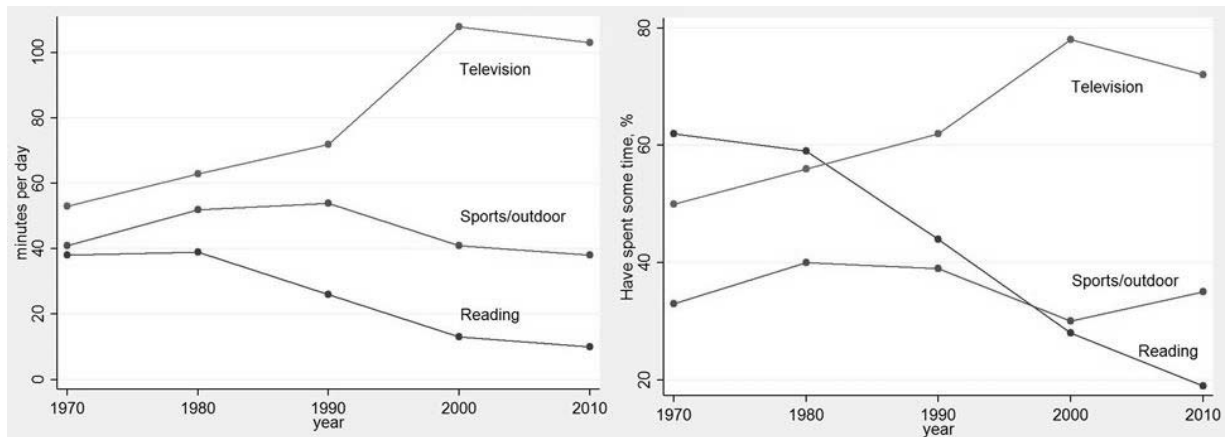


Figure 4. Time used on selected leisure activities by men aged 16-24 an average day.
 Left: Minutes spent an average day. Watching television; sports or outdoor activities; reading.
 Right: Percent of people who spent some time on the activity an average day.

8 Conclusion

In this paper, we have used the geographically staggered expansion of cable TV in Norway during the 1980's and 1990's to examine the impacts of access to commercial TV channels on children and youths. Our outcome measures include ability test scores (measured at age 18) and early career indicators, such as high school completion and disability benefit recipiency (at age 21). Our findings indicate that commercial TV affects ability test scores and high school completion rates *negatively*, and the risk of disability program entry *positively*. The estimated effects are moderately sized, but far from negligible. For example, one year of full cable television coverage lowers intelligence test scores by around 0.75 % of a standard deviation, corresponding to 0.1 IQ points. This is roughly 3 % of the previously estimated impact of one year extra schooling (Brinch and Galloway, 2012). The same increase in cable TV reduces the high-school completion rate at age 21 by around half a percentage point.

While our point estimates should be interpreted with some care – given a considerable degree of statistical uncertainty, the qualitative nature of our main findings are extremely robust. We perform a large number of robustness analyses, all confirming the negative impacts on cognitive ability and high-school completion. The negative impacts are largest for TV exposure around age 6-12; i.e., during primary school age. And they are larger for low-income than for high-income households.

It is probable that the developmental impacts of increased supply of TV entertainment during childhood depend both on the extent to which the actual consumption of entertainment

is regulated by parents and on the kind of activities TV consumption substitutes for. These factors vary across households, and may explain why there apparently is a social gradient in the negative cognitive/educational effects. Data from time-use-surveys indicate that the increase in TV consumption on average substituted for reading; hence, one may hypothesize that the negative effects identified in our paper do not stem from increases in TV consumption *per se*, but rather by the resultant reduction in reading activities.

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Appendix

Table 8. Descriptive statistics, selected cohorts

	Year of birth							
	1974	1976	1978	1980	1982	1984	1986	1987
	mean	mean	mean	mean	mean	mean	mean	mean
Outcomes								
ability test score at age 18	5.25	5.25	5.24	5.15	5.16	5.10	5.05	4.99
high school completed at age 21	0.66	0.69	0.68	0.67	0.68	0.66	0.64	0.65
in work or education at age 21	0.83	0.87	0.89	0.89	0.89	0.88	0.90	0.90
disability reception at age 21	0.02	0.03	0.03	0.03	0.04	0.04	0.04	0.04
Explanatory variable								
TV, years of coverage until age 18	1.99	2.41	2.81	3.28	3.77	4.28	4.87	5.07
Controls								
Individual-level								
education father, four levels	1.90	1.95	1.99	2.01	2.00	2.01	2.01	2.00
education mother, four levels	1.79	1.83	1.85	1.84	1.82	1.82	1.84	1.85
income father, 1000 USD	64.70	67.59	67.37	70.70	65.94	66.88	75.38	74.58
income mother, 1000 USD	13.39	15.28	17.49	21.08	21.56	23.52	28.93	29.69
Municipality-level								
schoolyears attained	11.16	11.26	11.35	11.45	11.52	11.59	11.67	11.69
income males, 1000 USD	59.87	62.31	61.14	64.31	59.60	59.85	67.18	66.16
income females, 1000 USD	18.21	21.01	22.66	25.75	25.73	27.45	33.10	33.60
employment rate males	0.79	0.80	0.81	0.81	0.79	0.78	0.80	0.80
employment rate females	0.31	0.35	0.39	0.45	0.46	0.48	0.56	0.57
schoolyears attained 1980	11.47	11.46	11.45	11.45	11.45	11.45	11.47	11.47
income males 1980, 1000 USD	64.43	64.49	64.43	64.31	64.36	64.33	64.66	64.72
income females 1980, 1000 USD	26.06	25.96	25.86	25.75	25.75	25.77	26.04	26.06
employment rate males 1980	0.81	0.81	0.82	0.81	0.82	0.81	0.82	0.82
employment rate females 1980	0.45	0.45	0.45	0.45	0.45	0.45	0.45	0.45
Observations	23860	21643	21939	21893	22207	21764	22870	23171

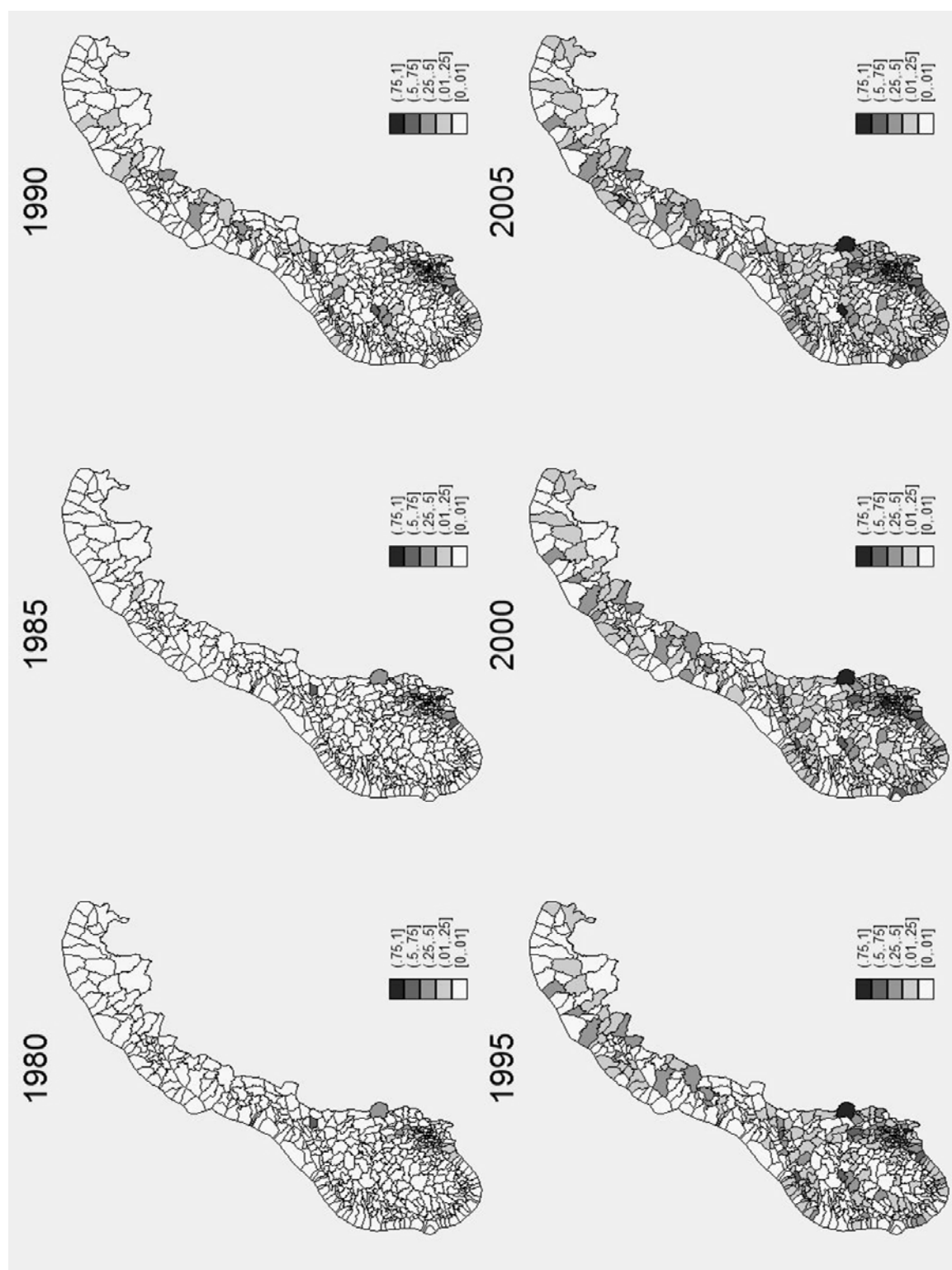


Figure 5. Cable television coverage by municipality, 1980-2005

Chapter II

Crisis and work effort: Sickness absence in the aftermath of terrorism*

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Abstract

I analyze how sickness absence behavior in Norwegian municipalities was affected by the terrorist attack in Norway on July 22, 2011. Using the geographical variation of victims' home municipality and register data covering the complete Norwegian population in a difference-in-differences approach, I find that sickness absence rates declined substantially in municipalities affected more intensely by the attack. In municipalities from which a resident was killed in the attack, sickness absence rates declined by 4 % compared to municipalities without victims. The effect is precisely estimated, stable across specifications including municipality-specific trends or time trends based on pre-attack demographics, does not show up in placebo regressions on other time periods, and persists for as long as there is available data. The results do not seem to be driven by an effect on labor market composition or traditional measures of social capital. The effect for people in their 20's is more than twice that for the population at large – for this group, local exposure to the attack decreased absence rates by around 10 %. As most of the victims were teenagers or young adults, this supports a mechanism where people were influenced by personally identifying with the victims.

*Data from Statistics Norway and Kommunedatabasen, a database on Norwegian regional data administered by the Norwegian Social Science Data Services (NSD), has been essential for this project. Statistics Norway and NSD are not responsible for the analysis of the data or for the interpretations in the present article.

1 Introduction

Do crises induce effort? Hard times may motivate people to work harder because their economic well-being is threatened. However, a crisis may foster effort even without altering economic fundamentals. In this paper I provide evidence on the latter case.

Terrorist attacks typically leave the economic structures of society largely intact, and thereby provide a setting to test economic mechanisms operating through other channels. On July 22, 2011 Norway was struck by a terrorist attack in which a total of 77 people were killed. I analyze the effect of local exposure to the attack on sickness absence behavior. Sickness absence can in this case be seen as a measure of work effort, since the attack for most people did not alter the physical determinants of actual sickness. Another advantage with sickness absenteeism is that it is something an individual can impact by his or her own without depending on anyone else.

Simply comparing aggregate outcomes before and after the attack might be misleading, since many other things may also have happened in the meantime. For example, there are large seasonal variations in sickness absence. A key point in my analysis is that although the country as a whole was deeply affected by the attack, people were personally impacted to an unequal degree.

The attack was directed at the Norwegian government generally, and at the Labor party in particular. The perpetrator placed a bomb in the government quarters in downtown Oslo before he went on to the summer camp of the Norwegian Labor Party's youth organization, a 45 minute drive and a short boat trip from Oslo. The summer camp is a traditional yearly gathering of Labor youth party members from around the country and always takes place on the island of Utøya. Most of the victims (69) were participants at the summer camp. The fact that the victims came from different parts of the country allows me to identify how people in a local community responded to the killing of one of the members of that community. In particular, I analyze how absence levels were affected in victims' and survivors' municipalities, using other municipalities as a control group. This means that common shocks like seasonal variations will be differenced out, and that I do not estimate national-level effects, but rather the additional response of local communities.

I find that the 2011 terrorist attack lead to a substantial reduction in sickness

absence in municipalities affected more severely by the attack. In municipalities that lost an inhabitant, the sickness absence rate fell by 0.25 percentage points in the time following the attack. In municipalities which had inhabitants present on Utøya, the main site of the attack, but lost none, the sickness absence rate fell by 0.16 percentage points. Since the average absence rate is 6.10 % in this time period, these effects correspond to drops in the absence rate of 4.0 % and 2.5 %, respectively. The results are robust to controlling for relevant municipality characteristics and several types of unobserved time trends.

Consistent with a mechanism where people are influenced by personally identifying with the victims (average age 22, median 18), the effect for people in their 20's is more than twice that for the population at large. For this group, local exposure to the attack decreased absence rates by around 10 %.

2 Related literature

In thinking about the impact of traumatic events on sickness absence, it is useful to distinguish between effects that might increase or decrease absence.

Physical and psychological injuries will of course increase sickness absence, and even though extra funds aimed at helping close relatives and survivors were allocated to the municipalities a short time after the attacks, studies have documented elevated post-traumatic stress among both survivors from Utøya (Dyb et al., 2014) and those that were present at work in the government buildings in Oslo when the bomb exploded (Hansen et al., 2013). In general, the death of a close relative is also associated with an increase in sickness absence (Markussen et al., 2011). Thus absence levels will likely have increased for those experiencing the attack first or second hand.

Individuals not experiencing the attack first or second hand may certainly also be traumatized, however for this much larger group it is not clear that the absence-increasing effects will dominate. Research on the 9/11, 2001 attack in the US has found that the attack in some ways lead to increased societal cohesion, as measured by trust and support for the government (Chanley, 2002; Traugott et al., 2002; Ford et al., 2003; Abramson et al., 2007). Likewise, Norwegian researchers have found higher reported trust levels in Norway after the 2011 attack than before (Wollebæk et al., 2012). A widely used measure of social

capital is blood donations - in the afternoon and evening of the July 22 attack, hundreds of people showed up at the blood bank in Oslo wanting to donate blood (Akkök, 2011), indicating that the will to contribute to the common good was indeed stirred.

Relatedly, some studies have documented a link between exposure to violence and political participation (Laufer and Solomon, 2006; Blattman, 2009; Hersh, 2013). These authors all relate their findings to the psychological literature on “post-traumatic growth,” defined as “the experience of positive change that occurs as a result of the struggle with highly challenging life crises (Tedeschi and Calhoun, 2004, p.1).” This fits particularly well with the narrative about the effects of the attack on young people that has dominated in Norwegian society – “generation Utøya” has come to mean the adolescent generation who was supposedly shocked into a more serious outlook on life by seeing so many of their peers being brutally executed.

The assumption that there is scope for a reduction in absence by such forces and that sickness absence in this context can be taken as a measure of effort is supported by the literature showing that there is indeed an element of choice in sickness absence behavior, as evidenced by studies of the effects of financial incentives (Johansson and Palme, 2005; Ziebarth and Karlsson, 2010), monitoring (D’Amuri, 2011; De Paola et al., 2014), group interaction effects (Ichino and Maggi, 2000; Bradley et al., 2007), and employment protection (Ichino and Riphahn, 2005; Olsson, 2009).

Since I employ municipality-level data I will estimate the average of the opposing effects.

3 Data

Data on victims come from two sources: The home municipalities of the 77 deceased victims are collected from Stormark (2011). In total 52 municipalities had at least one inhabitant killed. The main site of the attack was the island of Utøya. There 66 people were injured, while 585 survived without physical harm (Norwegian Directorate of Health, 2012). Data on which municipalities from which there were inhabitants present on Utøya has been acquired from the 18 County Governors, who disbursed funds targeted at following up these indi-

viduals in their respective municipalities. I know that in total 127 municipalities had surviving inhabitants, but I do not have information about the number of survivors from individual municipalities.

Quarterly data on physician-certified sickness absence come from administrative registers from Statistics Norway covering the full Norwegian population. The definition of the sickness absence rate is “man-days lost due to own sickness as a percentage of contractual man-days.” The rest of the municipality data is also based on population registers, and is extracted from *Kommunedatabasen*, a database on Norwegian regional data administered by the Norwegian Social Science Data Services (NSD). Five of the country’s 428 municipalities are dropped because of inconsistent time series caused by municipality mergers.

Table (1) shows descriptive statistics for several pre-attack municipality-level variables by future treatment status. The column with the heading *T1* comprises municipalities from which an inhabitant was killed in the attack, while *T2* covers municipalities that lost none, but that had at least one inhabitant was present at the main site of the attack. Thus, *T1* and *T2* are mutually exclusive.

There are some systematic differences between the three groups. First, the *T1* group has a higher level of sickness absence than the two others. Second, there is considerable oversampling of large and densely populated municipalities into the treatment groups. This is not surprising, in that such municipalities likely had more participants, both because of the larger population itself and because more centralized municipalities offer easier travel routes. The three groups are highly similar with regards to other variables.

Although what is important for my purposes is that the trends of the groups balance, since all permanent differences will be absorbed by the municipality fixed effects, I include a discussion in the appendix of where the difference in pre-level sickness absence levels might come from. The conclusion is that they seem to be related to more people participating from urban municipalities, which tend to have somewhat higher absence rates than more rural municipalities in the same area.

Table 1: Descriptive statistics 2010 (pre-treatment) by future treatment status

	T1		T2		Control	
	mean	sd	mean	sd	mean	sd
sickness absence rate, %	6.29	(1.024)	6.04	(1.012)	5.97	(1.251)
population, 1000	36.76	(84.464)	15.57	(25.911)	3.94	(3.770)
population density	0.72	(0.206)	0.62	(0.254)	0.41	(0.259)
income (USD 1000)	50.96	(5.164)	50.26	(5.076)	46.99	(4.196)
unemployment males, %	2.74	(0.530)	2.48	(0.692)	2.60	(1.122)
public sector, share of labor force	0.21	(0.058)	0.20	(0.050)	0.25	(0.069)
health and social, share of labor force	0.21	(0.031)	0.20	(0.032)	0.21	(0.035)
female workers, share of labor force	0.47	(0.012)	0.47	(0.013)	0.46	(0.015)
education, average years of schooling	11.94	(0.426)	11.80	(0.348)	11.55	(0.321)
disability recipients, share of inhabitants	0.07	(0.016)	0.07	(0.017)	0.07	(0.022)
welfare recipients, share of inhabitants	0.03	(0.008)	0.02	(0.007)	0.03	(0.011)
Labor party vote share 2007	0.31	(0.098)	0.30	(0.111)	0.30	(0.144)
Labor party vote share 2009	0.37	(0.085)	0.36	(0.086)	0.34	(0.091)
Progress party vote share 2007	0.18	(0.078)	0.16	(0.094)	0.10	(0.096)
Progress party vote share 2009	0.24	(0.054)	0.23	(0.060)	0.22	(0.061)
<i>N</i>	52		122		249	

Note: Sickness absence rate measures “man-days lost due to own sickness as a percentage of contractual man-days;” population density is the share of inhabitants in a municipality living in an “urban settlement” (a hub of buildings inhabited by at least 200 persons, with a minimum distance of 50 m between buildings); income is average gross income of all persons aged 17 or more, converted to USD with an exchange rate of 1 USD=6.5 NOK; unemployment is percentage of men with no earnings, averaged through the year; public sector counts share of workers employed in municipal or state administration; health and social denotes share of workers employed in health and social services, both public and private.

4 Empirical strategy

The identification comes from the geographical distribution of participants and victims. I employ a difference-in-differences design with two treatments varying among municipalities: Whether one or more individuals from the municipality were killed in the attack ($T1$); and whether one or more people were present on Utøya, but of whom none were killed ($T2$). Thus the treatments are mutually exclusive. Since $T1$ can be considered similar to, but more intense than $T2$, I expect the effects to have the same direction, but the effects of $T1$ to be stronger.

The identifying assumption is that conditional on area and time fixed effects, participation at the camp and the identity of the victims are uncorrelated with determinants of sickness absence. This assumption will be challenged in several

ways.

I assume that people living in a municipality from which someone was killed or survived received a higher degree of exposure to the event than people living in other municipalities. The most direct channel would have been direct and indirect personal connections and public events. In the election survey after the 2011 election, respondents from treated municipalities were more likely to have attended a public event related to the attack afterwards than others.

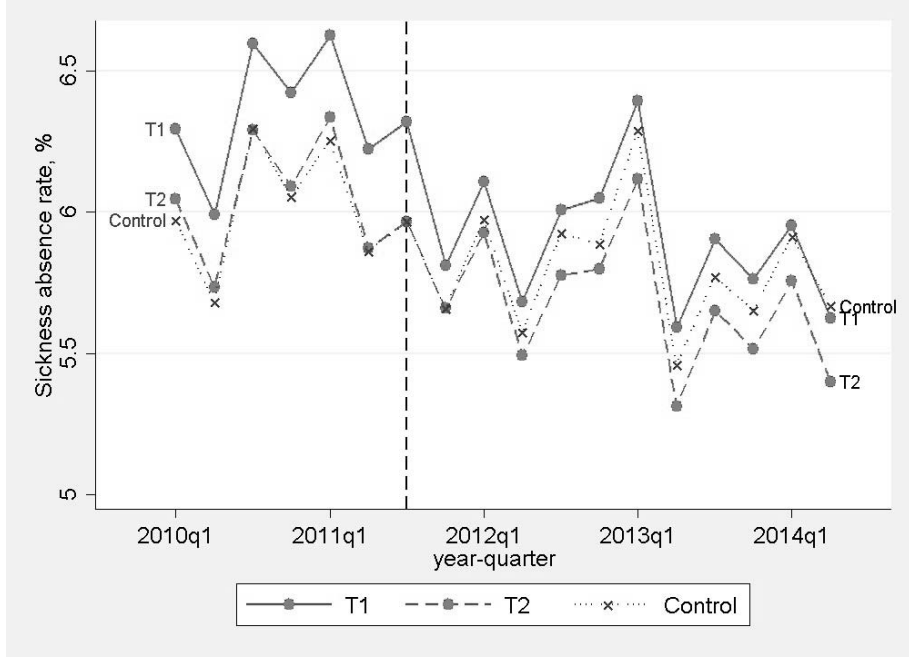
Media coverage also probably played a role: There is a substantial share of people who report to read district and local newspapers (Statistics Norway, 2014), of which the country has almost 200 (Høst, 2012). More generally, local identity is strong in Norway. This can be evidenced by high turnout in municipal elections (60-65 %) and the fact that most within country migration takes place within municipality boundaries (Statistics Norway, 2013). In a 2004 survey, only around half of respondents felt an affiliation (“tilhørighet”) to people in their neighboring municipalities (Frisvoll and Almås, 2004). The fact that news about individuals is routinely accompanied by information about their municipality of residence also indicates the perception that this information captures a salient feature of a person’s identity. Such reporting was the case with the July 22 attack as well, and in fact lists of victims and basic information about them, such as name, age, and home municipality, continue to be maintained online by two of the major news outlets to this day (NRK, 2015; Verdens Gang, 2015).

4.1 Time trends

It is essential for the difference-in-differences design that time trends are parallel, I therefore show in Figure (1) the time trends in the pre- and post-period. I later estimate on a time series from 2008, but show the trends from 2010 here for visibility. The trends for the period 2008-2014, shown in Figure (4) in the appendix, follow the same pattern.

From Figure (1) alone it is possible to see the basic findings. The groups clearly follow very similar trends before the attack. The dashed, vertical line indicates the quarter in which the attack took place, after which the level of the T1 group visibly drops relative to the others, while the T2 group exhibits a more delayed response.

Figure 1: Sickness absence levels 2010 - 2014 by quarter and treatment status. Vertical line indicates the quarter in which the terrorist attack took place.



4.2 Empirical model

I estimate the following linear baseline empirical model:

$$y_{mt} = \beta_1 T1_{mt} + \beta_2 T2_{mt} + \delta_m + \gamma_t + \epsilon_{mt} \quad (1)$$

y_{mt} denotes sickness absence rate in percent; $T1$ and $T2$ are indicator variables which are 0 for all time periods up to and including the third quarter of 2011, then switch to 1 in the remaining periods for those municipalities that received the respective treatments. Whether or not to include the quarter in which the attack took place makes essentially no difference for the results. $T1$ indicates that at least one inhabitant of the municipality was killed in the attack, $T2$ that at least one inhabitant of the municipality was present on Utøya, but no inhabitants were killed. Thus $T1$ and $T2$ are mutually exclusive.

All regressions will include municipality fixed effects δ_m and time (year x quarter) fixed effects γ_t and cluster standard errors at municipality. As robustness checks I include several time-varying municipality covariates, interactions between time trends and the 2010-levels of the municipality covariates, and

municipality-specific linear time trends.

5 Results

5.1 Baseline results and robustness checks

Table (2) displays the basic results and robustness tests. The first column shows the results from the baseline specification with no controls other than time and municipality fixed effects. The sickness absence rate dropped by 0.25 percentage points after the attack in municipalities from which an inhabitant was killed, and 0.16 percentage points in the less intensely treated municipalities from which an inhabitant participated, but survived.

Adding a list of covariates relevant to sickness absence in the second column does not have a large impact on the results, as these covariates do not vary very much and municipality fixed effects are already included. More stringent robustness tests are given by the third and fourth columns, which adds municipality-specific linear time trends (3) or linear time trends interacted with the 2010-levels of relevant covariates (4). I include population and population density in the latter trend specification, as we saw from the descriptive statistics that the treatment and control groups differed substantially on these measures. This specification allows the municipalities to follow different trends depending on the pre-attack levels of these characteristics. I also include the vote share of the Labor party in the 2009 election in a similar way to capture whether there were particular developments taking place in municipalities characterized by high (or low) support for the Labor party, which itself of course was hit severely by the attack. The results are robust across specifications.

The baseline results mean that sickness absence declined by 4.0 % from the average of 6.10 % in the most intensively treated group, and by 2.6 % in the second treatment group. Since the effects on those closest to the victims are almost certainly absence-increasing and there are spillovers between municipalities because of personal relationships, the true absence-decreasing effects are likely even larger.

The absolute size of the estimated coefficients are very similar when considering women and men separately, ref. Table 6 in the appendix. Since women as a group have almost three percentage points higher absence rate than men (7.6 %

vs. 4.9 %), this means that the percent reduction was much larger for men than women, 5.0 % as opposed to 3.2 %.

Table 2: Effects on sickness absence—main results and robustness checks

	(1) b/se	(2) b/se	(3) b/se	(4) b/se
T1	-0.245*** (0.066)	-0.243*** (0.067)	-0.209** (0.092)	-0.211*** (0.070)
T2	-0.161*** (0.058)	-0.153*** (0.057)	-0.103 (0.079)	-0.138** (0.058)
population density	No	Yes	Yes	Yes
population	No	Yes	Yes	Yes
schoolyears	No	Yes	Yes	Yes
health social sector share	No	Yes	Yes	Yes
female share of labor force	No	Yes	Yes	Yes
population#time trend	No	No	No	Yes
pop.density#time trend	No	No	No	Yes
Labor party vote (2009)#time trend	No	No	No	Yes
municipality-specific time trends	No	No	Yes	No
year#quarter f.e.	Yes	Yes	Yes	Yes
municip. f.e.	Yes	Yes	Yes	Yes
R-sqr	0.242	0.244	0.380	0.248
N	10152	10152	10152	10152
# municipalities	423	423	423	423
depvar mean	6.10	6.10	6.10	6.10
depvar sd	1.24	1.24	1.24	1.24

Note: Sickness absence is measured in percent. Point estimates are to be interpreted as effects in percentage points. In specification with covariate#time trend, population and population density are measured in 2010, Labor party vote in 2009 election. Time period 2008q1-2013q4. Standard errors clustered at municipality. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.2 Dynamics

To investigate the dynamics of the effects I include lags and leads for the timing of the attack. Time t is the quarter in which the attack took place, i.e. the third quarter of 2011, and denotes a variable that takes the value 1 in this quarter for those municipalities that received treatment $T1$ and 0 at all other times. I include three further quarters back ($t-1$, $t-2$, $t-3$) to have a full year, and lags as long as the data go, until the second quarter in 2014 ($t+1$ to $t+11$). The

logic is the same for treatment $T2$. The results are reported in Table (8) in the appendix. The estimated treatment coefficients from the baseline specification (containing only municipality and time fixed effects) are plotted graphically in figures (2) and (3).

The leads serve as a check on the assumption that the two groups followed similar trends. It is reassuring that the coefficients on the leaded shocks are all quite precisely centered on zero.

Given that much of the third quarter of 2011 took place after the attack, one may wonder why there seems to be no effect in that quarter. The main explanation is that long absence spells (more than one month) are behind 2/3 of the overall absence rate, thus any intervention not leading to early return from spells that have already started is bound to operate with a significant lag. In addition, it is possible that there were initial absence-increasing effects which counteracted the absence-decreasing effects; or that the effects worked through long-term absenteeism, and thus needed some time to respond. Both these explanations may be true, and although I cannot disentangle them with the present data, there are some observations that suggest the importance of the impact through long-term absenteeism: First is the continued, gradual decline, which would follow readily from a reduction in new long-term spells. Second, when analyzing data on spell duration at the national level and controlling only for year and quarter fixed effects separately, very long spells ($>$ one quarter) is the category showing a significant decline in the post-treatment period.

From $t + 1$ onwards, the drop of about 0.2 percentage points for treatment $T1$ sets in immediately and shows signs of becoming even larger with time.

The response to treatment $T2$ shows a somewhat different pattern. Consistent with treatment $T2$ being less intense, the absence rate reacts less dramatically in the short run, but with time approaches the response of the $T1$ group, with an effect of about 0.2 percentage points, suggesting that the underlying slow-moving mechanics are much the same. The estimated coefficients are graphed in figure (3).

The results from the analysis of the dynamics of the effects supports the basic identification strategy and show that the effects are highly persistent. The persistence indicates that people do not increase their presence at work out of a demand for information or of curiosity, forces that would surely dissipate with

time.

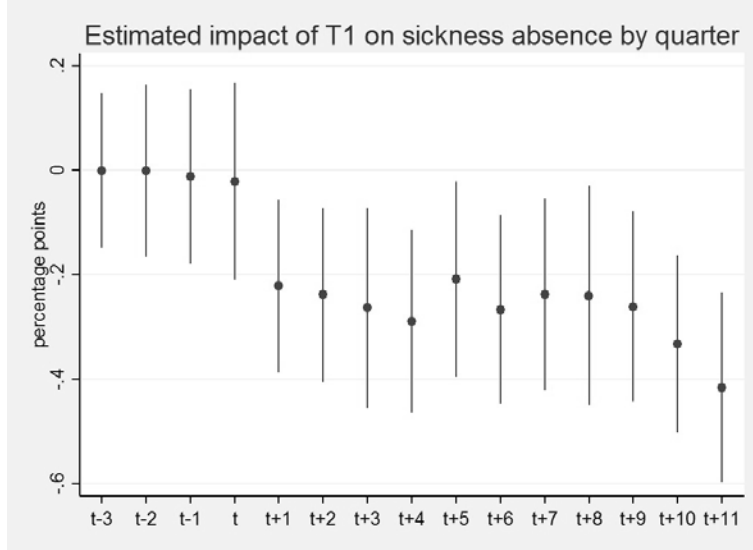


Figure 2: Impact of T1 by quarter. Estimated coefficients from “Baseline” specification in Table (8). Time t is the quarter in which the attack took place, i.e. the third quarter of 2011. Each bar displays the estimated effect of a one-period treatment taking place in the time period indicated on the horizontal axis. Vertical bars indicate 95 % confidence intervals.

5.3 Population size

The treatments take place at the municipality level. To investigate the role played by population size, I here weight observations by population and estimate on subsamples defined by population size.

Table (8) shows the results from regressions where observations are weighted by municipality working age population. Column (1) presents the baseline model with municipality and time fixed effects, but now weighting observations. Weighting reduces the size of the estimated coefficients in the baseline specification. Since the treatments are plausibly weaker in larger populations, this is not surprising. Because of the presence of a few very large municipalities, I include regressions where I exclude (2) the largest municipality (Oslo), (3) the three largest (Oslo, Bergen, Trondheim), (4) municipalities with working age population above 50 000, and finally (5) municipalities with working age population of 25 000 or above. As I exclude the largest municipalities, the results quickly approach the unweighted estimates from Section 5.1.

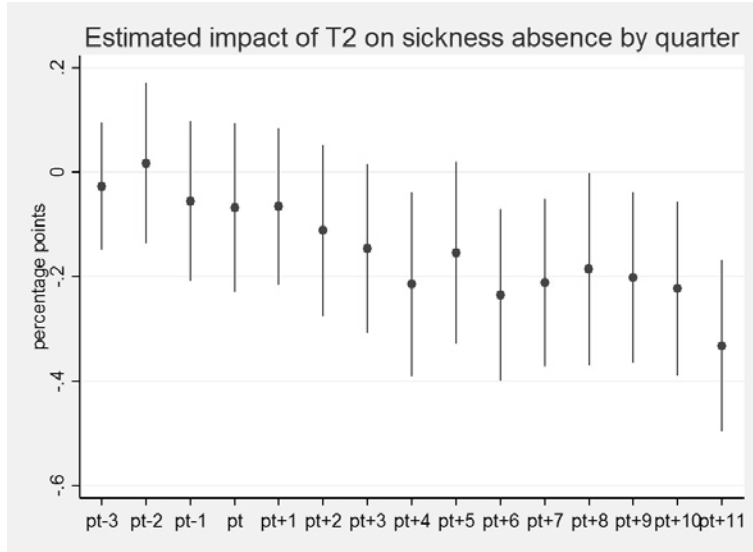


Figure 3: Impact of T2 by quarter. Estimated coefficients from “Baseline” specification in Table (8). Time t is the quarter in which the attack took place, i.e. the third quarter of 2011. Each bar displays the estimated effect of a one-period treatment taking place in the time period indicated on the horizontal axis. Vertical bars indicate 95 % confidence intervals.

Table (9) shows the results from estimations on subsamples of municipalities with between 10 000 and 20 000 inhabitants and fewer than 10 000 inhabitants. The results confirm that the effect was somewhat stronger in smaller municipalities. I do not include a subsample of municipalities with more than 20 000 inhabitants, as only two of these municipalities did not receive any treatment.

The overall picture is that once very large municipalities are excluded, the effects seem to depend on population size in a quite limited way, suggesting that though personal connections probably did play a role in the treatments, so did more general municipality-wide factors.

5.4 Log-linear specification

The simple linear model does not make assumptions about the relationship between the effect and the starting value. Table (10) provides the results from regressions with the natural logarithm of the sickness absence rate as dependent variable, which corresponds to assuming that the relative change is the same across starting levels. The main difference is that the effect of $T2$ here seems more comparable to that of $T1$, however, in general the results are quite in line

with those from the linear model, with coefficients between -0.037 and -0.03, and -0.03 and -0.025 for the two groups, respectively.

5.5 Placebo test on 2001-2007

As a placebo test I move the time window from 2008-2013 to the up to now unused 2001-2007 and introduce the treatments on the corresponding points on the timeline. I.e. instead of the treatments taking place in the third quarter of 2011, they now occur in the third quarter of 2004. The results are shown in Table (11). The estimated coefficients are very close to zero across all specifications.

6 Mechanism

In this section I investigate several possible channels for the results. First, I find no evidence that the large downward shift in sickness absence are due to an effect on labor market composition. Second, analyzing various measures of social capital, I again find that these do not explain the results. Third, I disaggregate sickness absence by age groups. From the analysis by age groups, a distinct finding emerges, which is that the youngest workers responded much stronger than older ones, i.e. their absence rates declined more. I take this as support for the hypothesis that personal identification with the victims played a substantial role.

6.1 Labor force composition

The persistence of the effects may suggest an explanation in terms of a shift in labor force composition. Below I analyze the effects on disability and labor market participation. The results are in Tables (12) and (13) in the appendix.

One possible mechanism is that people with a high propensity for absence sorted into disability after the attack. Using quarterly data from 2010 onwards I do not find any effect on the disability rate. The point estimates are small and even negative in most of the specifications.

Another possibility is that high-absence individuals dropped out of the labor market completely, so it is reasonable to investigate labor force participation generally. Here the picture is somewhat ambiguous. The baseline specification (only

municipality and time fixed effects) gives a positive estimate, while introducing covariates brings the estimate down to zero. Overall, there are no clear signs that labor force participation is the mechanism.

I find no evidence that an effect on the composition of workers is driving the results.

6.2 Social capital

As referenced in section 2, studies in political science and psychology have found associations between shocks such as terrorist attacks and subsequent increases in various measures of social capital. To investigate whether this might be a relevant mechanism for the results on work effort, I analyze the effect on turnout in elections, donations to charity, membership in organizations, and levels of trust, all of which have been seen as measures of social capital (Knack and Keefer, 1997; Guiso et al., 2004). The results are given in table 3 below.

Using election data from the local elections 1999-2011, the effects of the treatments are precisely estimated at 0, as shown in the first column. In the second column I also analyze the average amount donated to “TV-aksjonen”, an annual fundraising event taking place in October. In 2011 the funds donated went to Norwegian People’s Aid and their work on removing mines and bombs in former war zones. The unit is dollars donated per capita, and as can be seen, the point estimates are very small. Both these measures are population-level data.

Using the official surveys after the elections in 2003, 2007, and 2011 allows me to perform analyses at the individual level. The surveys contained questions about membership in organizations and trust. The point estimates for membership in some organization are positive, while those for trust are negative.

Overall, no consistent pattern of how these traditional measures of social capital were affected appears, and point estimates are generally small and point in different directions.

6.3 Effect heterogeneity by age

In Section 5 we saw that the effects of the two treatments on the overall absence rate were precisely estimated at around -0.25 and -0.16, or -3.9 and -2.6 %, and highly robust across model specifications. However, if the underlying mechanism

Table 3: Effect on social capital

	Turnout (Population) b/se	Charity (Population) b/se	Member (Survey) b/se	Trust (Survey) b/se
T1	0.004 (0.005)	0.006 (0.159)	0.020 (0.055)	-0.076 (0.324)
T2	0.004 (0.003)	-0.145 (0.144)	0.038 (0.046)	-0.619** (0.261)
year f.e.	Yes	Yes	Yes	Yes
municipality f.e.	Yes	Yes	Yes	Yes
ymean	0.64	7.14	0.62	6.75
R-sqr	0.859	0.853	0.097	0.196
N	1720	3825	4597	3097

Note: Turnout is municipality-level turnout in the municipal elections 1999, 2003, 2007, 2011. Charity is dollars donated per capita in the fundraising event “TV-aksjonen,” yearly 2003-2013. Member is a dummy variable indicating whether the respondent is a member of an organization or not, Trust indicates level of agreement with the statement that most people can be trusted (10=agrees). Years 2003-2007-2011. Standard errors clustered on municipality. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

is one where people are identifying with the victims and evaluating their own situation in comparison with them, we should not expect the effects to be uniform across the age distribution. In particular, since a notorious feature of the attack was the young age of the victims (the average age of the victims was 22), we should expect the effects to be larger for the youngest workers.

In Table (4) I divide the working population into nine age groups. The estimated effects are largest for people in their 20’s, whose absence was reduced by up to 0.5 percentage points. When taking into account the very different underlying means of the age groups, the young stand out even more strikingly. The two bottom rows display the percent change for each of the groups. We see that among workers closest to the victims’ age, i.e. those aged 20-24, absence rates dropped by 11 and 9 % from the the two treatments, respectively. Consistent with expectations, the effects are also quite strong, though somewhat lower, for people towards the end of their 20’s, while broadly leveling off after that to around the pooled effects.

There is invariably more noise in these estimates, as each observation results

from aggregating over a smaller number of individuals. Nevertheless, the results are substantial, consistent, and for the most part quite precise. For some age groups, some municipality-quarters are missing for certain small municipalities as numbers are not published if they are the result of too few individuals. In Table (4) these observations are treated as missing at random. To provide a check on this, Table (14) in the appendix excludes all such municipalities entirely when looking at each age group. The general picture is the same.

To sum up, when disaggregating by age groups, there is a clear finding that the reduction in sickness absence was much larger among young people than among older ones, supporting the popular notion that identifying with the victims played an important role.

Table 4: Effect on sickness absence – by age group.

	age20-24 b/se	age25-29 b/se	age30-34 b/se	age35-39 b/se	age40-44 b/se	age45-49 b/se	age50-54 b/se	age55-59 b/se	age60-66 b/se
T1	-0.482 (0.121) ***	-0.371 (0.136) ***	-0.156 (0.140)	-0.326 (0.129) **	-0.215 (0.117) *	-0.164 (0.124)	-0.224 (0.123) *	-0.267 (0.144) *	-0.329 (0.181) *
T2	-0.379 (0.109) ***	-0.465 (0.120) ***	0.083 (0.122)	-0.089 (0.128)	-0.142 (0.111)	-0.326 (0.115) ***	-0.155 (0.123)	-0.057 (0.135)	-0.168 (0.180)
y-q f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
municip. f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-sqr	0.084	0.066	0.058	0.037	0.046	0.038	0.041	0.044	0.070
N	10918	10929	10945	10976	10983	10987	10992	10985	10987
# municip.	423	423	423	423	423	423	423	423	423
depvar mean	4.23	5.61	6.09	5.99	5.92	5.99	6.40	6.98	8.31
depvar sd	2.00	2.31	2.33	2.16	1.97	2.01	2.20	2.38	3.02
% change									
T1	-11.4	-6.6	-2.6	-5.4	-3.6	-2.7	-3.5	-3.8	-4.0
T2	-9.0	-8.3	1.4	-1.5	-2.4	-5.4	-2.4	-0.8	-2.0

Note: Sickness absence is measured in percent. Estimations on subsamples of age groups. Time period 2008q1-2014q2. Standard errors clustered on municipality. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

7 Conclusion

In this paper, I have analyzed workers' response to the July 22, 2011 terrorist attack in Norway. I find that the sickness absence rate in municipalities from which an inhabitant was killed in the attack declined substantially after the attack took place. The decline was around one quarter of a percentage point, implying a percent decline of 3.9. In municipalities from which no one was killed in the

attack, but that had someone who or experienced it first-hand, there was also a substantial decline, of around 2.5 %.

The estimated effects are highly robust across model specifications. I investigate several possible underlying mechanisms. I find no evidence of explanations in terms of labor market composition on traditional measures of social capital, but differential response between age groups stand out. In particular, the decline in the absence rate for people in their 20's is twice that for people in other age groups. This supports the popular notion that young people were particularly impacted by the attack because of belonging to the same generation as the victims.

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8 Appendix

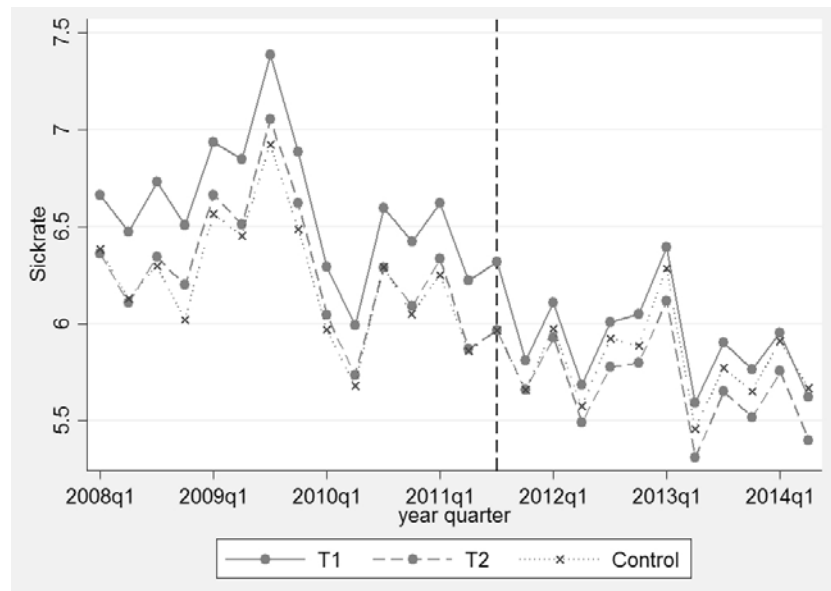
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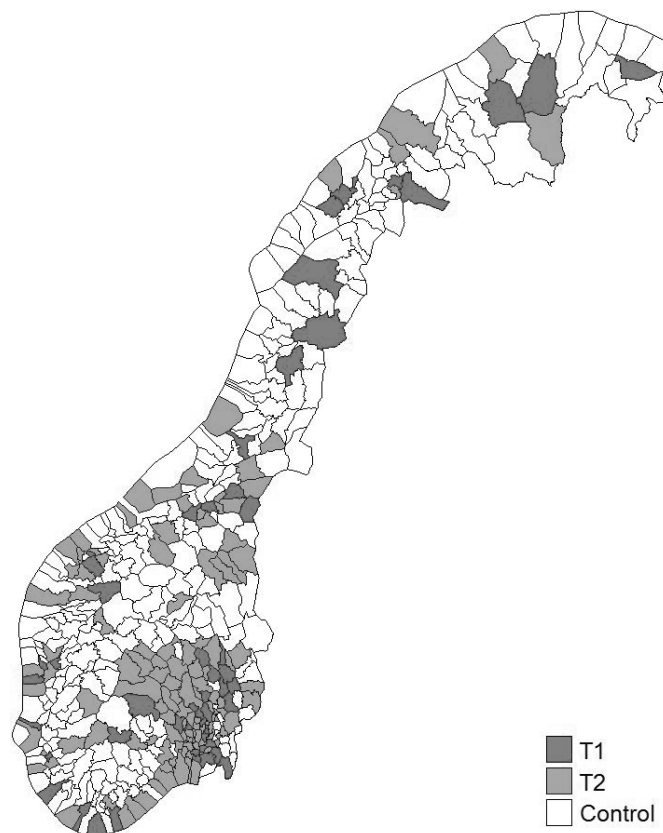
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Figure 4: Sickness absence 2008 - 2014 by quarter



Note: Sickness absence rate in percent. The large drop in 2009–2010 was associated with a severe outbreak of swine flu in 2009, and public attention directed at the high sickness absence level around the same time because of a proposed reform of a part of the welfare system (*IA-avtalen*) (Nossen, 2011; Norwegian Institute of Public Health, 2015). These factors affected the whole country, and are absorbed by time fixed effects in my regressions.

Figure 5: Home municipalities in the July 22 attack



Pre-attack difference in levels

Table (5) shows the results of regressing pre-treatment levels of sickness absence on future treatment status. As can be seen from the first two columns, the difference in pre-levels is statistically significant, and remains so after controls for municipality population and density, the two other covariates conspicuously differing between the groups in Table (1), are added.

However, the large regional differences in population and population density that exist in Norway mean that these variables are not necessarily comparable across the country. One way to take this into account while still allowing for selection into camp participation based on settlement patterns is to interact population density with fixed effects for each of the 19 counties of the country. The third column shows the results of letting the coefficient on density differ across the 19 counties in this way. In the fourth column the same is done also for population. In the latter two specifications, the group level differences are no longer statistically significant, suggesting that these measure of urbanity indeed underlie both higher absence levels and selection into the treatment groups, most likely by capturing ease of travel.

Table 5: Pre-treatment absence levels and future treatment status.

	1	2	3	4
	b/se	b/se	b/se	b/se
T1	0.366** (0.148)	0.458*** (0.156)	0.219 (0.147)	0.213 (0.163)
T2	0.054 (0.111)	0.085 (0.121)	0.042 (0.108)	0.014 (0.112)
population	No	Yes	No	No
density	No	Yes	No	No
county-density	No	No	Yes	Yes
county-pop	No	No	No	Yes
year#quarter f.e.	Yes	Yes	Yes	Yes
R-sqr	0.070	0.076	0.256	0.285
N	6345	6345	6345	6345
# municipalities	423	423	423	423
depvar mean	6.29	6.29	6.29	6.29
depvar sd	1.26	1.26	1.26	1.26

Note: Sickness absence rate in percent. Time period 2008q1-2011q2. Standard errors clustered at municipality. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Effect on sickness absence by gender. Women (top), men (bottom).

Women	Baseline b/se	Cov b/se	Ind.trends b/se	Cov-trends b/se
T1	-0.242*** (0.092)	-0.237** (0.094)	-0.227* (0.132)	-0.208** (0.095)
T2	-0.167** (0.082)	-0.162** (0.082)	-0.165 (0.122)	-0.148* (0.083)
population density	No	Yes	Yes	Yes
population	No	Yes	Yes	Yes
schoolyears	No	Yes	Yes	Yes
health social sector share	No	Yes	Yes	Yes
female share of labor force	No	Yes	Yes	Yes
population#time trend	No	No	No	Yes
pop.density#time trend	No	No	No	Yes
Labor party vote#time trend	No	No	No	Yes
municipality-specific time trends	No	No	Yes	No
year#quarter f.e.	Yes	Yes	Yes	Yes
municip. f.e.	Yes	Yes	Yes	Yes
R-sqr	0.118	0.121	0.257	0.123
N	10152	10152	10152	10152
# municipalities	423	423	423	423
depvar mean	7.62	7.62	7.62	7.62
depvar sd	1.57	1.57	1.57	1.57
Men	Baseline b/se	Cov b/se	Ind.trends b/se	Cov-trends b/se
T1	-0.244*** (0.064)	-0.243*** (0.065)	-0.184* (0.097)	-0.210*** (0.065)
T2	-0.154** (0.063)	-0.148** (0.062)	-0.053 (0.092)	-0.132** (0.062)
population density	No	Yes	Yes	Yes
population	No	Yes	Yes	Yes
schoolyears	No	Yes	Yes	Yes
health social sector share	No	Yes	Yes	Yes
female share of labor force	No	Yes	Yes	Yes
population#time trend	No	No	No	Yes
pop.density#time trend	No	No	No	Yes
Labor party vote#time trend	No	No	No	Yes
municipality-specific time trends	No	No	Yes	No
year#quarter f.e.	Yes	Yes	Yes	Yes
municip. f.e.	Yes	Yes	Yes	Yes
R-sqr	0.206	0.207	0.329	0.211
N	10152	10151	10151	10151
# municipalities	423	423	423	423
depvar mean	4.89	4.89	4.89	4.89
depvar sd	1.29	1.29	1.29	1.29

Note: Separate regressions for women and men. In specification with covariate#time trend, population and population density are measured in 2010, Labor party vote in 2009 election. Time period 2008q1-2013q4. Standard errors clustered at municipality. * p<0.10, ** p<0.05, *** p<0.01

Table 7: Effects on sickness absence - dynamics

	Baseline b/se	Cov b/se	Ind.trends b/se	Cov-trends b/se
T1, t-3	-0.000 (0.076)	-0.006 (0.076)	0.018 (0.091)	0.008 (0.077)
T1, t-2	-0.000 (0.083)	-0.010 (0.085)	0.029 (0.107)	0.007 (0.083)
T1, t-1	-0.012 (0.085)	-0.022 (0.086)	0.019 (0.126)	-0.002 (0.085)
T1, t	-0.021 (0.096)	-0.031 (0.096)	0.012 (0.138)	-0.009 (0.097)
T1, t+1	-0.221*** (0.084)	-0.231*** (0.085)	-0.187 (0.137)	-0.207** (0.086)
T1, t+2	-0.238*** (0.085)	-0.232*** (0.087)	-0.181 (0.152)	-0.203** (0.087)
T1, t+3	-0.263*** (0.098)	-0.257*** (0.098)	-0.204 (0.174)	-0.226** (0.101)
T1, t+4	-0.289*** (0.089)	-0.282*** (0.090)	-0.228 (0.174)	-0.250*** (0.092)
T1, t+5	-0.209** (0.095)	-0.202** (0.095)	-0.146 (0.192)	-0.167* (0.100)
T1, t+6	-0.267*** (0.092)	-0.273*** (0.095)	-0.195 (0.198)	-0.235** (0.098)
T1, t+7	-0.238** (0.093)	-0.244** (0.097)	-0.165 (0.211)	-0.204** (0.100)
T1, t+8	-0.240** (0.107)	-0.246** (0.110)	-0.165 (0.215)	-0.204* (0.115)
T1, t+9	-0.261*** (0.093)	-0.267*** (0.095)	-0.185 (0.225)	-0.223** (0.104)
T1, t+10	-0.333*** (0.087)	0.000 (.)	0.000 (.)	0.000 (.)
T1, t+11	-0.416*** (0.092)	0.000 (.)	0.000 (.)	0.000 (.)
T2, t-3	-0.027 (0.062)	-0.027 (0.063)	-0.049 (0.073)	-0.020 (0.063)
T2, t-2	0.018 (0.078)	0.017 (0.078)	-0.002 (0.095)	0.025 (0.077)
T2, t-1	-0.055 (0.078)	-0.056 (0.077)	-0.078 (0.102)	-0.047 (0.076)
T2, t	-0.068 (0.082)	-0.068 (0.082)	-0.094 (0.110)	-0.058 (0.082)
T2, t+1	-0.065 (0.076)	-0.066 (0.076)	-0.094 (0.113)	-0.054 (0.076)
T2, t+2	-0.111 (0.083)	-0.100 (0.084)	-0.136 (0.128)	-0.087 (0.085)
T2, t+3	-0.146* (0.082)	-0.134* (0.081)	-0.173 (0.140)	-0.121 (0.081)
T2, t+4	-0.215** (0.090)	-0.203** (0.089)	-0.245 (0.153)	-0.188** (0.089)
T2, t+5	-0.154* (0.089)	-0.142 (0.088)	-0.187 (0.158)	-0.126 (0.089)
T2, t+6	-0.235*** (0.084)	-0.231*** (0.084)	-0.267 (0.168)	-0.215** (0.085)
T2, t+7	-0.211*** (0.082)	-0.208** (0.082)	-0.247 (0.174)	-0.190** (0.083)
T2, t+8	-0.185** (0.094)	-0.182* (0.093)	-0.224 (0.187)	-0.163* (0.096)
T2, t+9	-0.202** (0.083)	-0.198** (0.083)	-0.243 (0.193)	-0.178** (0.088)
T2, t+10	-0.223*** (0.085)	0.000 (.)	0.000 (.)	0.000 (.)
T2, t+11	-0.332*** (0.083)	0.000 (.)	0.000 (.)	0.000 (.)
population density	No	Yes	Yes	Yes
population	No	Yes	Yes	Yes
schoolyears	No	Yes	Yes	Yes
health social sector share	No	Yes	Yes	Yes
female share of labor force	No	Yes	Yes	Yes
population#time trend	No	No	No	Yes
pop.density#time trend	No	No	No	Yes
Labor party vote#time trend	No	No	No	Yes
municipality-specific time trends	No	No	Yes	No
year#quarter f.e.	Yes	Yes	Yes	Yes
municip. f.e.	Yes	Yes	Yes	Yes
R-sqr	0.243	0.245	0.380	0.249
N	10998	10152	10152	10152
# municipalities	423	423	423	423
depvar mean	6.07	6.10	6.10	6.10
depvar sd	1.23	1.24	1.24	1.24

Note: Sickness absence in percent. Treatments in one period (quarter) only. Time period 2008q1-2013q4. Standard errors clustered at municipality. * p<0.10, ** p<0.05, *** p<0.01

Table 8: Effect on sickness absence - weighted regressions

	(1)	(2)	(3)	(4)	(5)
	Baseline	exclOslo	pop<100000	pop<50000	pop<25000
	b/se	b/se	b/se	b/se	b/se
T1	-0.099** (0.048)	-0.131** (0.053)	-0.124** (0.057)	-0.177*** (0.057)	-0.200*** (0.059)
T2	-0.099** (0.042)	-0.099** (0.042)	-0.103** (0.044)	-0.103** (0.044)	-0.137*** (0.048)
year#quarter f.e.	Yes	Yes	Yes	Yes	Yes
municip f.e.	Yes	Yes	Yes	Yes	Yes
R-sqr	0.564	0.534	0.510	0.500	0.450
N	10152	10128	10080	10008	9648
# municipalities	423	422	420	417	402
depvar mean	5.90	6.02	6.05	6.16	6.22
depvar sd	1.01	1.02	1.07	1.03	1.05

Note: Weights are number of people employed in 2010. Columns (2)–(5) show estimations on subsamples created by excluding: (2) Oslo, (3) the three largest (Oslo, Bergen, Trondheim), (4) municipalities with working age population above 50 000, and (5) municipalities with working age population of 25 000 or above. Time period 2008q1-2013q4. Standard errors clustered at municipality. * p<0.10, ** p<0.05, *** p<0.01

Table 9: Effect on sickness absence - by population subsamples.

	(1)	(2)
	10000-20000	pop<10000
	b/se	b/se
T1	-0.217** (0.105)	-0.284** (0.122)
T2	-0.264** (0.100)	-0.108 (0.076)
year#quarter f.e.	Yes	Yes
municip f.e.	Yes	Yes
R-sqr	0.889	0.670
N	1368	7608
municipalities	57	317
depvar mean	6.27	6.08
depvar sd	1.01	1.31

Note: Sickness absence in percent. Non-weighted regressions. Columns (1) and (2) show estimations on subsamples of municipalities with working age population (in 2010) in intervals (10000–20000) and (0-10000), respectively. Time period 2008q1-2013q4. Standard errors clustered at municipality. * p<0.10, ** p<0.05, *** p<0.01

Table 10: Effects on log of sickness absence.
Time period 2007q4-2013q3. Standard errors clustered at municipality.

	Baseline b/se	Cov b/se	Ind.trends b/se	Cov-trends b/se
T1	-0.037*** (0.011)	-0.035*** (0.011)	-0.035** (0.015)	-0.030*** (0.011)
T2	-0.030*** (0.010)	-0.028*** (0.010)	-0.024* (0.013)	-0.025** (0.010)
population density	No	Yes	Yes	Yes
population	No	Yes	Yes	Yes
schoolyears	No	Yes	Yes	Yes
health social sector share	No	Yes	Yes	Yes
female share of labor force	No	Yes	Yes	Yes
population#time trend	No	No	No	Yes
pop.density#time trend	No	No	No	Yes
Labor party vote#time trend	No	No	No	Yes
municipality-specific time trends	No	No	Yes	No
year#quarter f.e.	Yes	Yes	Yes	Yes
municip. f.e.	Yes	Yes	Yes	Yes
R-sqr	0.228	0.231	0.363	0.233
N	10152	10152	10152	10152
# municipalities	423	423	423	423
depvar mean	1.79	1.79	1.79	1.79
depvar sd	0.21	0.21	0.21	0.21

Note: Sickness absence is measured in log of the absence rate. In specification with covariate#time trend, population and population density are measured in 2010, Labor party vote in 2009 election. Time period 2008q1-2013q4. Standard errors clustered at municipality. * p<0.10, ** p<0.05, *** p<0.01

Table 11: Placebo test on an earlier time period

	Baseline b/se	Cov b/se	Ind.trends b/se	Cov-trends b/se
T1	0.007 (0.073)	-0.013 (0.075)	0.019 (0.108)	0.025 (0.079)
T2	-0.017 (0.065)	-0.038 (0.067)	-0.079 (0.095)	-0.013 (0.071)
population density	No	Yes	Yes	Yes
population	No	Yes	Yes	Yes
schoolyears	No	Yes	Yes	Yes
health social sector share	No	Yes	Yes	Yes
female share of labor force	No	Yes	Yes	Yes
population#time trend	No	No	No	Yes
pop.density#time trend	No	No	No	Yes
Labor party vote#time trend	No	No	No	Yes
municipality-specific time trends	No	No	Yes	No
year#quarter f.e.	Yes	Yes	Yes	Yes
municip. f.e.	Yes	Yes	Yes	Yes
R-sqr	0.439	0.441	0.539	0.443
N	10152	10152	10152	10152
# municipalities	423	423	423	423
depvar mean	6.57	6.57	6.57	6.57
depvar sd	1.52	1.52	1.52	1.52

Note: Sickness absence is measured in percent. In specification with covariate#time trend, population, population density, and Labor party vote are measured in 2003. Time period 2001q1-2007q4. Standard errors clustered at municipality. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Effect on disability rate

	Baseline b/se	Cov b/se	Ind.trends b/se	Cov-trends b/se
T1	-0.000 (0.043)	0.020 (0.042)	0.037 (0.042)	-0.050 (0.041)
T2	-0.026 (0.033)	-0.025 (0.032)	-0.007 (0.032)	-0.070** (0.028)
population density	No	Yes	Yes	Yes
population	No	Yes	Yes	Yes
schoolyears	No	Yes	Yes	Yes
health social sector share	No	Yes	Yes	Yes
female share of labor force	No	Yes	Yes	Yes
population#time trend	No	No	No	Yes
pop.density#time trend	No	No	No	Yes
Labor party vote#time trend	No	No	No	Yes
municipality-specific time trends	No	No	Yes	No
year#quarter f.e.	Yes	Yes	Yes	Yes
municip. f.e.	Yes	Yes	Yes	Yes
R-sqr	0.088	0.112	0.125	0.614
N	4653	4653	4653	4653
# municipalities	423	423	423	423
depvar mean	7.07	7.07	7.07	7.07
depvar sd	2.10	2.10	2.10	2.10

Note: Disability recipients in percent of population. In specification with covariate#time trend, population and population density are measured in 2010, Labor party vote in 2009 election. Time period 2010q4-2013q4. Standard errors clustered at municipality. * p<0.10, ** p<0.05, *** p<0.01

Table 13: Effects on labor force participation

	Baseline b/se	Cov b/se	Ind.trends b/se	Cov-trends b/se
T1	0.037 (0.347)	0.001 (0.352)	-0.262 (0.530)	0.139 (0.366)
T2	0.004 (0.318)	0.042 (0.314)	-0.445 (0.528)	0.104 (0.330)
population density	No	Yes	Yes	Yes
population	No	Yes	Yes	Yes
schoolyears	No	Yes	Yes	Yes
health social sector share	No	Yes	Yes	Yes
female share of labor force	No	Yes	Yes	Yes
population#time trend	No	No	No	Yes
pop.density#time trend	No	No	No	Yes
Labor party vote#time trend	No	No	No	Yes
municipality-specific time trends	No	No	Yes	No
year#quarter f.e.	Yes	Yes	Yes	Yes
municip. f.e.	Yes	Yes	Yes	Yes
R-sqr	0.384	0.385	0.427	0.386
N	10152	10152	10152	10152
# municipalities	423	423	423	423
depvar mean	61.27	61.27	61.27	61.27
depvar sd	9.25	9.25	9.25	9.25

Note: Labor force participation as a percentage of the population. In specification with covariate#time trend, population and population density are measured in 2010, Labor party vote in 2009 election. Time period 2000-2012, 2011 excluded. Standard errors clustered at municipality. * p<0.10, ** p<0.05, *** p<0.01

Table 14: Effect on sickness absence – by age group. Excluding municipalities with some missing observations.

	age20-24 b/se	age25-29 b/se	age30-34 b/se	age35-39 b/se	age40-44 b/se	age45-49 b/se	age50-54 b/se	age55-59 b/se	age60-66 b/se
T1	-0.473 (0.118) ***	-0.306 (0.131) **	-0.170 (0.138)	-0.276 (0.124) **	-0.209 (0.117) *	-0.150 (0.118)	-0.191 (0.119)	-0.230 (0.143)	-0.297 (0.178) *
T2	-0.396 (0.106) ***	-0.403 (0.114) ***	0.063 (0.121)	-0.038 (0.123)	-0.125 (0.111)	-0.297 (0.109) ***	-0.099 (0.119)	-0.033 (0.134)	-0.151 (0.176)
y-q f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
municip. f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-sqr	0.095	0.074	0.061	0.042	0.047	0.038	0.044	0.045	0.073
N	10827	10773	10908	11205	11340	11286	11313	11313	11313
# municip.	401	399	404	415	420	418	419	419	419
depvar mean	4.28	5.66	6.12	6.00	5.92	5.99	6.38	6.99	8.30
depvar sd	1.90	2.21	2.23	2.10	1.95	1.96	2.13	2.33	2.92

Note: Sickness absence is measured in percent. Estimations on subsamples of age groups. Time period 2008q1-2014q2. Standard errors clustered on municipality. * p<0.10, ** p<0.05, *** p<0.01

Chapter III:

Do parents create voting habits in their children?

Evidence from a natural experiment

Øystein M. Hernæs*

Abstract

I exploit variation among first-time voters that arises through voting eligibility rules and two-year election cycles to estimate the effects of first-time voting environment on turnout in Norwegian elections. I find that obtaining the right to vote at a lower age is associated with substantially higher turnout among first-time voters, and that this is driven by parental influence. Counter to conventional wisdom about the habitual nature of voting, this difference in political participation does not persist for subsequent elections.

Keywords: Elections, Voting Behavior

JEL Classification: D72

*European University Institute, Department of Economics, email: oeystein.hernaes@eui.eu. (Some of) the data applied in the analysis in this publication are based on ‘Norwegian Election Study, 1993’, ‘Election Survey 1997’, ‘Election Survey 2009’, ‘Election Survey for the Municipal Elections, 1995’, ‘Local Election Survey 1999’, ‘Local Elections 2003,’ and ‘Samordnet levekårsundersøkelse 1996–2009’. The surveys were financed by Institute of Social Research (ISF), Department of Political Science, University of Oslo and the Ministry of Regional Development and Local Government. The data are provided by Statistics Norway, and prepared and made available by the Norwegian Social Science Data Services (NSD). Neither ISF, the Ministry of Regional Development and Local Government, Statistics Norway, Department of Political Science, University of Oslo nor NSD are responsible for the analyses/interpretation of the data presented here.

1 Introduction

Do parents create voting habits in their children?

In many countries, there is an ongoing debate about what the legal voting age should be.¹The question is usually whether to lower the age requirement from 18 to 16 years. In the Norwegian case, the explicitly stated key objective for lowering the requirement is to increase the political participation of young people, in both the short and the long run, and it is believed that lowering the voting age will accomplish this (Norwegian Ministry of Local Government and Regional Development, 2008). This is actually an evidence-based view, based on research about habit-formation in voting and the assumption that it will be feasible to channel young people into actually participating (Norwegian Ministry of Local Government and Regional Development (2006, pp. 43–44), Norwegian Ministry of Local Government and Regional Development (2008, pp. 54–55)). Another objective is to have a larger share of the population represented politically.

This paper investigates the effects of voting for the first time when being relatively young, and sheds light on the mechanisms behind individual voting behavior.

The methodology is to exploit variation in first-time voting environment that arises through voting eligibility rules and two-year election cycles, which lead first-time voters to come from two clearly separated cohorts – those that turn 18 years old in the election year and those that turn 19. Age trends for the two types are depicted in Figure 1. I analyze first what causes the large discrepancy between the two leftmost data points, second whether there is a difference between the rest of the two turnout profiles.

I find that among first-time voters, the youngest cohort has higher turnout, and that this is driven by the influence of parents. However, in the long run, no trace remains of this initial difference. This suggests that the source of one's turnout decision matters for habit formation in political participation, and that lowering the voting age is no panacea for inducing higher turnout.

¹For instance in Austria, Germany, Ireland, the Nordic countries, UK, US (Council of Europe, 2011).

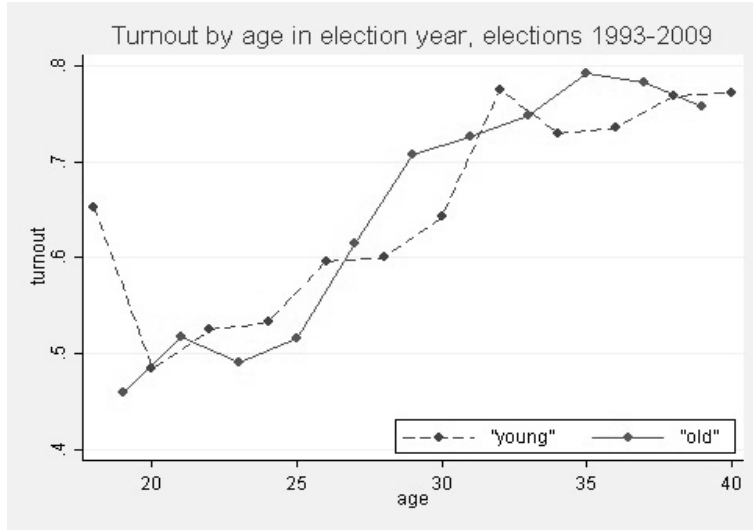


Figure 1: Average turnout by cohort. “young” were able to vote for the first time in the year they turned 18, “old” in the year they turned 19. Age obtained in the election year on the horizontal axis.

2 Literature

Voting behavior has been widely studied, but identification of causal effects has been a challenge. The contributions most closely related come from the strand of literature that investigates voting habits. Franklin (2004) claims that the extension of the franchise by lowering the voting age from 21 to 18 years in the US lead to a first-time voting situation that was less conducive to acquiring a habit of voting, and that the result was many life-long non-voters.

Meredith (2009) compares the voting behavior of individuals who were just eligible to vote in a previous election and individuals who fell just short of being eligible. Eligibility increases subsequent turnout and to some extent the likelihoods both of being affiliated with a party and of this party being the Democrats. The data come from the California Statewide Voter File, which contains information of all registered voters in the state of California. Relatedly, Dinas (2012) exploits the fact that in the 1968 US presidential election, the voting eligibility requirement was to have turned 21 years old on election day. Using survey data, he finds that those that were just eligible in the 1968 election have a substantially higher propensity to vote in the 1970 and 1972 elections than those not eligible in 1968. These studies compare individuals who are of essentially the same age but become eligible at different points in time, whereas I compare people that

are of different ages, but become eligible at the same point in time. Further, in Meredith (2009) and Dinas (2012) 's studies, those that are compared always differ in the number of elections they have been eligible to participate in, whereas in my study, individuals that are compared have participated in the same number of elections, but differ in one year of age and thereby in the character of the previous election experience.

Denny and Doyle (2009) take individual persistence in turnout as given, and investigate whether the persistence is due to individual characteristics or habits. The data used is from a British child development study, which contains information on respondents' turnout in three elections as well as many other factors. The authors use residential mobility between the ages of 16 and 23 as an instrumental variable that has a (negative) effect on turnout when the individual is in that age range, but no direct effect on later turnout. They estimate that everything else being equal, an individual who voted in one election is 13 percentage points more likely to vote in the next election.

Experimental work by Gerber et al. (2003) shows that people randomly urged to vote were more likely to vote in both the upcoming and the subsequent election. The authors use the experimental treatments as instrumental variables, and find substantial 2SLS estimates of voting in one election on the probability of voting in the subsequent election.

A compelling recent study by Fujiwara et al. (2013) estimate habit formation by exploiting the fact that rainfall on election day reduces turnout. Employing county-level data on US presidential elections 1952-2012, they use lagged precipitation as an instrument for lagged turnout, and estimate that a 1-percentage point decrease in turnout reduces turnout in the next election by as much as 0.7-0.9 percentage points.

Overall, the studies with credible identification strategies tend to find substantial effects of voting in one election on voting in the next election. This has provided a rationale for policy-makers for lowering the age limit in order to counteract declining turnout trends, since young people would then be "pulled into" voting and start developing the "habit of voting" earlier.

3 Data

After every Norwegian election, Statistics Norway initiates a survey of the electorate by means of a representative sample. A (stratified) sample of around 4000 people between the ages of 17 and 80 years is drawn. The election councils in the municipalities then check in the electoral rolls whether these persons voted, after which it is attempted to undertake an extensive interview, lasting approximately one hour, with everyone in the sample.

These surveys have been carried out after all parliamentary elections since 1957 and local elections since 1995. Unfortunately, only some of the surveys contain information on the year of birth of an individual (as opposed to age at the time of the interview), which is essential for my purposes. For parliamentary elections, information about year of birth is available for 1993, 1997 and 2009; for local elections, information about year of birth is available for 1995, 1999 and 2003.

The gross sample consists of all individuals drawn by Statistics Norway. An advantage with the gross sample is that there is no selection into or out of it. Individuals from gross samples make up 70 % of my final file. For some of the elections, it is only the net sample that is available. The net sample consists of those that accept to be interviewed. It contains information that may be used to control for variables that are known to be important for turnout as well as other political variables. The survey is undertaken as a rolling panel, where half the initial gross sample in one survey is retained from the previous survey. Because of the rolling panel structure, the data is potentially plagued by an observer effect, as being interviewed about one election may affect behavior related to the next election. I deal with this by excluding all second-time participants from the net samples.

The combined file contains 6683 first-time survey participants born after 1959. The reason for the age cut-off is that before 1978 the voting age was 20 years, thus I use only those born in 1960 or later, as there may be different effects at play in 18 vs. 19 year-olds than in 20 vs. 21 year-olds.

Summary statistics are presented in table (1). The variables that are observed for everyone in the sample are *voted*, *age*, *female*, and *election year*. Answers to the interview questions are not observed for all participants, because questions have varied somewhat from year to year and because the table contains data from

both the gross and the net samples.

voted, *female* and *in school* are coded as dummy variables. *education* is in three levels – completed less than high school, completed high school, or completed tertiary education. The answers to the rest of the interview questions are originally in four levels, but recoded to a 0-1 scale for ease of interpretation. The first column contains all voters, the second only first-time voters, i.e. those aged 18 or 19 in the election year, while the final two columns break down the first-time voters further into 18- and 19-year olds.

As a preview of some of the later analysis, we may note the gap in turnout between the two groups of first-time voters displayed in the first row (in the two final columns) – those aged 18 voted almost 20 percentage points more than their one year older first-time voting peers. This is somewhat puzzling, in light of the well-established positive relationship between age and turnout (Wolfinger and Rosenstone, 1980). A natural follow-up question is whether this might be related to the other large discrepancy between the groups – the fact that 18 year-olds are in school 21 percentage points more. I will answer this in the negative below.

4 Empirical strategy

Turnout is usually seen as depending on a host of factors, such as education, income, benefit of election outcome, cost of voting, information, civic duty, and so on. I consider a margin that cuts across most of these factors, in particular – I analyze the effect of age at first election, both in that first election and in later elections.

I exploit the variation in voting age that arises through the two-year election cycles. In most countries the requirement for voting is to be at least 18 years old on the day of the election. In Norway, however, the requirement is to become 18 years old in the year of the election. Since elections take place in the fall every second year, a person can be anywhere between 17 years and 8 months (born December 31) and 19 years and 8 months (born January 1) the first time he or she has the opportunity to vote.

An advantage of the requirement of turning 18 years old in the year of the election is that any variation related to time of the year at which an individual is born can be ignored since both groups of comparison consist of people born

Table 1: Descriptive statistics

	All voters	First time voters		
		Pooled	age 18	age 19
voted	0.65	0.56	0.65	0.46
age	29.94	18.47	18.00	19.00
female	0.49	0.49	0.51	0.48
interview questions				
in school	0.24	0.59	0.68	0.47
education (in 3)	2.07	1.91	1.86	1.97
civic duty to vote	0.80	0.68	0.67	0.70
interest	0.52	0.52	0.53	0.51
discussed election	0.55	0.52	0.56	0.46
trust local politicians	0.45	0.45	0.43	0.46
trust national politic.	0.49	0.54	0.52	0.57
trust people	0.47	0.42	0.38	0.48
work hours	0.57	0.24	0.25	0.23
election year				
1993	0.07	0.13	0.15	0.11
1995	0.14	0.17	0.16	0.17
1997	0.17	0.14	0.13	0.17
1999	0.11	0.17	0.20	0.14
2003	0.27	0.20	0.20	0.21
2009	0.25	0.19	0.17	0.20
<i>N</i>	6683	650	343	307

Note: Means shown. Data from election surveys. *voted*, *female* and *in school* are coded as dummy variables. *education* is in three levels – completed less than high school, completed high school, or completed tertiary education. The answers to the rest of the interview questions are originally in four levels, but recoded to a 0-1 scale for ease of interpretation.

throughout the year and. Any time-of-the-year effects thus cancel out within cohorts.

I do not observe anyone more than once, but by knowing their year of birth I know how old they were in the year when they obtained the opportunity to vote for the first time. Since elections take place in odd years, it will simply be the case that those born in an odd year obtained the opportunity to vote for the first time in the year they turned 18, while those born in an even year obtained the opportunity the year they turned 19. I analyze whether the voting behavior

of these two groups differ, first only for first-time voters, secondly for all other voters.

I estimate the following linear probability model:

$$v_i = \alpha + \beta YOUNG_i + x_i\gamma + \epsilon_i \quad (1)$$

v_i is equal to 1 if the individual voted, 0 otherwise. $YOUNG_i$ is equal to 1 if the individual was eligible to vote in the year of turning 18 years old, 0 otherwise (in which case the individual was first eligible in the year of turning 19 years old). x_i denotes gender and the election year the individual was observed. ϵ_i is the disturbance term.

When analyzing only first-time-voters, I clearly cannot include any type of age effect, as this would be perfectly collinear with the *YOUNG*-dummy. When estimating effects on non-first-time-voters, however, x_i also includes a cubic function in age. In that case, two adjacent cohorts will differ in the same way in the early eligibility indicator $YOUNG_i$ and age. The identifying assumption is that the age effect can be accounted for by controlling flexibly for age in the estimation. Period effects should cancel out through the treatment taking place every other year.

Whether to vote or not is a binary choice. The linear probability model is reasonable here, as I am interested in the partial effect of early eligibility averaged over the other covariates, not at particular values, and the covariates are mostly discrete. I use robust standard errors because of heteroskedasticity arising from the binary dependent variable.

5 Results

5.1 First-time voters

Table (2) shows the result of regressing the voting dummy on whether an individual was eligible at 18 years old or not including only first-time voters. The first column contains all first-time voters in the sample. In average 18-year olds have a turnout that is almost 20 percentage points higher than that of the 19-year olds. The rest of this subsection investigates the mechanism behind this finding.

The second and third columns in table (2) restrict the sample to those inter-

view surveys in which there was a question of whether an individual was currently attending school. Thus we might test the hypothesis that the cohort difference is due to the 18-year olds being in school, where they through the curriculum or by teachers might be exposed more to the election and politics. From the second column we see that there is a substantial difference between the cohorts also in this sample. Although being in school is positively related to turnout, as can be seen in the third column, the estimated coefficient on the eligibility dummy changes very little when including this information. Thus the reason 18-year olds have higher turnout than 19-year olds is not because they are in school.

Table 2: Turnout-differences among first-time voters

	(1) b/se	(2) b/se	(3) b/se
YOUNG	0.192*** (0.038)	0.271*** (0.066)	0.254*** (0.069)
In school			0.073 (0.073)
ymean	0.56	0.54	0.54
R-sqr	0.083	0.078	0.083
N	650	218	218

Note: All models include gender and election year fixed effects and robust standard errors.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In Norway everyone can vote freely from anywhere, so the hypothesis that the results are due to it being more costly to vote if one has moved to another region and that the 19 year olds have done so to a greater extent can also be discarded (Government.no, 2015).

Another likely explanation for the difference between the two cohorts of first-time voters is that younger voters more often live at home and thus may be influenced to vote by their parents. This influence can take many forms, like social or conformity pressure, interest-inducing exposure, and practical reasons. Such an explanation is supported by a study on Danish first-time voters: Bhatti and Hansen (2012) use massive amounts of observational data and find a strong association between living with parents and turnout, even while controlling for a wide range of socioeconomic variables, including ongoing and completed educa-

tion, previous moving patterns, gender, marriage status, school grades, income, ethnicity, citizenship, and parents' age, education and income.

Unfortunately, there is no question in the Norwegian election surveys about where a respondent is living. I therefore use information from a separate living condition survey to assess this mechanism. The living conditions survey have information about young people's turnout and whether people were living with their parents for the 1997 and 2005 waves.

Table (3) shows the result of running the same regressions as before using only data on first-time voters from the living conditions surveys. The first column shows that we find the same pattern among 18 and 19 year olds as in the election surveys, here with the 18 year olds in average voting 25 percentage points more. This difference is substantially reduced when including information about whether an individual lived with his or her parents, as can be seen in column (2). The two final columns confirm the importance for turnout of living with one's parents as opposed to being in school.

It appears that what is driving the results is the fact that the 18 year olds to a greater extent live with their parents.

Table 3: Testing the living-with-parents hypothesis

	(1) b/se	(2) b/se	(3) b/se	(4) b/se
YOUNG	0.253* (0.137)	0.132 (0.134)	0.230 (0.144)	0.100 (0.139)
Living with parents		0.334*** (0.102)		0.340*** (0.101)
In school			0.068 (0.110)	0.091 (0.103)
ymean	0.64	0.64	0.64	0.64
R-sqr	0.050	0.146	0.055	0.154
N	101	101	101	101

Note: Data from living conditions survey. All models include gender and year fixed effects and robust standard errors.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

To investigate the mechanism in more detail, I return to data from the election surveys to analyze effects on other political variables. Table (4) shows the results from regressing six other political variables on the eligibility dummy *YOUNG*.

The six political variables are whether voting is considered a civic duty (*duty*), degree of political interest (*interest*), how much the respondent discussed the election (*discussed*), and degree of trust at three different political levels (*trust1-trust3*), all coded on a scale between 0 and 1.

The results show that for these measures, the only significant difference between the cohorts is the extent to which they discussed the election, with the younger voters having discussed the election substantially more. Although the statistical uncertainty is considerable here, these results suggest that parental influence works through social pressure or imitation rather than increasing the offspring's intrinsic motivation to vote.

Table 4: Testing the channel of influence

	(1)	(2)	(3)	(4)	(5)	(6)
	duty	interest	discussed	trust1	trust2	trust3
	b/se	b/se	b/se	b/se	b/se	b/se
YOUNG	-0.033 (0.095)	0.032 (0.025)	0.120*** (0.037)	-0.034 (0.039)	-0.055 (0.046)	-0.017 (0.047)
ymean	0.68	0.51	0.53	0.45	0.54	0.41
R-sqr	0.001	0.005	0.040	0.007	0.014	0.001
N	103	367	259	106	107	148

Note: Data from election surveys. The dependent variables measure (1) whether voting is considered a civic duty (*duty*), (2) degree of political interest (*interest*), (3) how much the respondent discussed the election (*discussed*), (4) degree of trust in politicians at the local level (*trust1*), (5) degree of trust in politicians and the national level (*trust2*), and (6) whether most people are trustworthy (*trust3*), all coded on a scale between 0 and 1. All models control for gender and year fixed effects and robust standard errors.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

To sum up, among the two cohorts of first-time voters, the youngest vote in elections substantially more frequently than the older ones, and this difference seems to be driven by the influence of parents. The next subsection investigates whether the cohort difference found for first-time voters is present also later in life.

5.2 Subsequent effects

So far I have demonstrated that early eligibility substantially increases turnout among first-time voters and that this is driven by parental influence. As persistence estimates in the literature are quite high – Gerber et al. (2003) find that a 1 percentage point increase in turnout increases turnout in the next election by 0.5 percentage points, and Fujiwara et al. (2013) find an even higher effect of between 0.7 and 0.9 percentage points in the next election – one would expect the cohort difference to persist also for future elections. As we will see below, this is not so.

Table (5) below shows the estimates of the average subsequent marginal effect of early eligibility. The results for first-time voters are provided as a reference in column (1), but first-time voters are left out when estimating the long-term effects, as I am here interested in the average long-term effects of being a young first-time voter and do not want to mix this up with what goes on in voters' first election. The second column includes a third degree polynomial in age to control flexibly for the age-on-voting profile. The estimate is very precisely close to 0. The third specification compares cohorts with the same election experience directly by including fixed effects for the number of elections for which an individual has been eligible to vote. Here the coefficient on early eligibility is slightly negative, as the YOUNG dummy is picking up the well-known long-term age gradient in turnout.

As an alternative to looking at the average over all later elections, one may analyze turnout in each cohort pair separately, i.e. the pairs formed by cohorts who have been eligible to vote the same number of times. Table (6) in the appendix shows the result of taking the specification with an individual's election experience, i.e. the number of times he or she has been eligible to go to the polls, from column (3) above, but now interacting the eligibility dummy YOUNG with fixed effects for election experience. Already in voters' second election, when the effect from the previous election would presumably be the strongest, the point estimate is close to 0. Later on the estimated coefficient on the interaction term is at times significantly negative, again likely because this is a period in the life where turnout is steadily increasing, and the cohort who could vote early in their first election will always be younger than the comparison cohort.

The linear probability model does not restrict the outcome to lie within the

Table 5: Subsequent effect of early eligibility on turnout

	First time voters	All other voters	
	(1)	(2)	(3)
	b/se	b/se	b/se
YOUNG	0.192***	-0.009	-0.024*
	(0.038)	(0.012)	(0.012)
age (cubic)	No	Yes	No
election experience f.e.	No	No	Yes
ymean	0.56	0.66	0.66
R-sqr	0.083	0.086	0.089
N	650	6033	6033

Note: Data from election surveys. All models include gender and election year fixed effects. “election experience” denotes number of times a voter has been eligible to vote. Robust standard errors. First-time voters excluded in columns (2)-(3).

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

unit interval, however it is reassuring to note that predicted turnout is never below 0, and in only one case does it exceed 1, in which case it is 1.02.

To conclude, I find no signs that the early high-turnout behavior induced by parents persists into adulthood. Perhaps this is not surprising, in light of the finding that the parental influence did not work through the young’s intrinsic motivation.

5.3 Implications of lowering the voting age: Turnout, Representation, Information

Although voting at a lower age leads to higher turnout among first-time voters, it has no effect for older voters. Thus lowering the voting age should not be regarded as a panacea for increasing long term turnout, even though it may lead to high youth turnout. This finding may suggest that voting is only habit-forming if it is construed as truly self-chosen by the individual.

An effect a lower voting age undoubtedly will have is to increase the relative representation of young people. The information value of the election may decrease or increase, depending on what one believes about the how well informed young voters are. Two additional cohorts would be voting, and to a relatively large extent, but some are afraid that these young voters would be less knowl-

edgeable and only contribute noise. A more optimistic view is that young voters also contribute information, in which case the information value of the election would improve.

Two caveats should be noted: dynamics may not be identical for younger voters, and it may be the case that a high turnout twice when young has long term effects.

6 Conclusions

In this paper I have used a natural experiment created by voting requirement rules by year of birth combined with two-year election cycles to analyze the effect of first-time voting environment on turnout.

I find that being eligible to vote for the first time at a lower age is associated with higher turnout in one's first election because of parental influence. However, despite previous findings of high persistence in political participation, those who were eligible to vote for the first time at a lower age do not display higher turnout in subsequent elections, thus demonstrating that the source of earlier political participation is important.

The results imply that lowering the voting age requirements should not be regarded as a panacea for increasing turnout in the long run, and suggest that the concept of autonomy should be taken seriously by parents and policy makers alike when trying to influence the political behavior of young people.

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Table 6: Subsequent effect–interaction with election experience

	All	
	b	se
2.elexp#YOUNG	-0.008	(0.040)
3.elexp#YOUNG	0.040	(0.045)
4.elexp#YOUNG	0.006	(0.044)
5.elexp#YOUNG	-0.088*	(0.041)
6.elexp#YOUNG	-0.095*	(0.040)
7.elexp#YOUNG	-0.086*	(0.039)
8.elexp#YOUNG	0.022	(0.037)
9.elexp#YOUNG	-0.061	(0.036)
10.elexp#YOUNG	-0.052	(0.038)
11.elexp#YOUNG	0.021	(0.045)
12.elexp#YOUNG	0.101	(0.054)
13.elexp#YOUNG	-0.047	(0.056)
14.elexp#YOUNG	0.074	(0.075)
15.elexp#YOUNG	-0.040	(0.069)
16.elexp#YOUNG	0.007	(0.079)
election experience f.e.	Yes	
ymean	0.66	
R-sqr	0.092	
N	6033	

Note: Data from election surveys. All models control gender and year fixed effects and robust standard errors. “election experience” denotes number of times a voter has been eligible to vote. First-time voters excluded.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$